

VOLUME 88 • NUMBER 5 • DECEMBER 1983

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The *American Historical Review* appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003 (202-544-2422) and is printed and mailed by the William Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228. The editorial offices are located in 914 Atwater, Indiana University, Bloomington, Indiana 47405 (812-335-7609).

The *AHR* is sent to members of the American Historical Association and to institutions holding subscriptions. Membership dues: For incomes of \$40,000 and above, \$60.00 annually; \$30,000–\$39,999, \$55.00; \$20,000–\$29,999, \$47.00; \$15,000–\$19,999, \$40.00; \$10,000–\$14,999, \$30.00; below \$10,000, students, and joint memberships \$20.00; associate (nonhistorian) \$25.00; life \$1,000. The proportion of dues allocated to the *AHR* is \$17.00. Subscription rates effective for volume 88: Class I, *American Historical Review* only, United States, Canada, and Mexico \$43.00, foreign \$47.00. Further information on membership, subscriptions, and the ordering of back issues is contained on the two pages—1(a) and 2(a)—immediately preceding the advertisements.

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HERBERT S. KLEIN is a professor of history at Columbia University. He has published extensively on Cuba, slavery, and the Atlantic slave trade, as well as on Andean society and the colonial economy of Spanish America. He is also interested in comparative analysis, most recently publishing an article on Italian immigrant integration in Argentina and the United States in the April 1983 issue of the *American Historical Review*. For many years he has worked closely with Stanley L. Engerman on demographic aspects of slavery and the slave trade. Currently, they are studying the economic aspects of emancipation in the Americas.

MICHAEL MCGIFFERT is the editor of the *William and Mary Quarterly*, published by the Institute of Early American History and Culture, and a professor of history at the College of William and Mary. He received his BA from Harvard, where Perry Miller sparked his interest in Puritanism, and the degrees of BD and PhD from Yale. His publications on the covenant divinity in early modern Britain and America include articles in the *New England Historical and Genealogical Register* (1976), the *Journal of British Studies* (1980), the *Journal of Ecclesiastical History* (1981), and the *Harvard Theological Review* (1982) as well as an introduction to his edition of Thomas Shepard's autobiography and journal, *God's Plot: The Paradoxes of Puritan Piety* (1972).

DONALD J. MATTHEISEN is an associate professor of history at the University of Lowell in Lowell, Massachusetts. He received his PhD in 1966 from the University of Minnesota. He has written several articles on the German revolution of 1848, including "Voters and Parliaments in the German Revolution of 1848: An Analysis of the Prussian Constituent Assembly," *Central European History* (1972), and "German Parliamentarianism in 1848: Roll-Call Voting in the Frankfurt Assembly," *Social Science History* (1981). His research interest in that revolution persists.

Articles

- God's Controversy with Jacobean England,
BY MICHAEL MCGIFFERT 1151
- Natives and Immigrants, Free Men and Slaves: Urban Workingmen
in the Antebellum South,
BY IRA BERLIN and HERBERT G. GUTMAN 1175
- The Level and Structure of Slave Prices on Cuban Plantations
in the Mid-Nineteenth Century: Some Comparative Perspectives,
BY STANLEY L. ENGERMAN, MANUEL MORENO FRAGINALS, and HERBERT S. KLEIN 1201

Review Essay

- History as Current Events: Recent Works on the
German Revolution of 1848,
BY DONALD J. MATTHEISEN 1219

Reviews of Books

GENERAL

- | | | | |
|---|------|--|------|
| WILLIAM MCKINLEY RUNYAN. <i>Life Histories and Psychobiography: Explorations in Theory and Method.</i>
By Fred Weinstein | 1238 | AILEEN KELLY. <i>Mikhail Bakunin: A Study in the Psychology and Politics of Utopianism.</i> By Paul Avrich | 1243 |
| YOSEF HAYIM YERUSHALMI. <i>Zakhor: Jewish History and Jewish Memory.</i> By Raphael Patai | 1239 | MICHAEL PODRO. <i>The Critical Historians of Art.</i>
By James Shedel | 1244 |
| WILLIAM H. MCNEILL. <i>The Pursuit of Power: Technology, Armed Force, and Society since A.D. 1000.</i>
By Gordon A. Craig | 1239 | EUGENE LUNN. <i>Marxism and Modernism: An Historical Study of Lukács, Brecht, Benjamin, and Adorno.</i>
By Jerrold Seigel | 1245 |
| EMMANUEL LE ROY LADURIE and JOSEPH GOY. <i>Tithe and Agrarian History from the Fourteenth to the Nineteenth Centuries: An Essay in Comparative History.</i> By Jan De Vries | 1240 | DOUGLAS KELLOGG WOOD. <i>Men against Time: Nicolas Berdyaev, T.S. Eliot, Aldous Huxley, and C. G. Jung.</i>
By Roland Stromberg | 1245 |
| PEREZ ZAGORIN. <i>Rebels and Rulers, 1500–1600.</i> Vol. 1, <i>Society, States, and Early Modern Revolution: Agrarian and Urban Rebellions</i> ; vol. 2, <i>Provincial Rebellion: Revolutionary Civil Wars, 1560–1660.</i> By Charles Tilly | 1241 | GORDON A. CRAIG and ALEXANDER L. GEORGE. <i>Force and Statecraft: Diplomatic Problems of Our Time.</i>
By Charles S. Maier | 1246 |
| JOHN CHILDS. <i>Armies and Warfare in Europe, 1648–1789.</i>
By Gunther E. Rothenberg | 1242 | GEOFFREY BARRACLOUGH. <i>From Agadir to Armageddon: Anatomy of a Crisis.</i> By Laurence Lafore | 1247 |
| DAVID HARVEY. <i>The Limits to Capital.</i>
By William H. Shaw | 1243 | ROBERT W. DESMOND. <i>Crisis and Conflict: World News Reporting between Two Wars, 1920–1940.</i>
By Modris Eksteins | 1248 |
| | | RICHARD L. LAEL. <i>The Yamashita Precedent: War Crimes and Command Responsibility.</i> By Peter Karsten | 1249 |

- DAVID ARMSTRONG. *The Rise of the International Organisation: A Short History*. By George W. Baer 1250

ANCIENT

- ERIK HORNUNG. *Conceptions of God in Ancient Egypt: The One and the Many*. By Donald B. Redford 1250
- ROBERT DREWS. *Basileus: The Evidence for Kingship in Geometric Greece*. By C. G. Thomas 1251
- NANCY H. DEMAND. *Thebes in the Fifth Century: Heracles Resurgent*. By Raphael Sealey 1251
- N. G. L. HAMMOND. *Alexander the Great: King, Commander, and Statesman*. By Christos C. Patsavos 1252
- HERBERT GRASSL. *Sozialökonomische Vorstellungen in der kaiserzeitlichen griechischen Literatur (1.-3. Jh. n. Chr.)*. By R. I. Frank 1253
- WAYNE A. MEEKS. *The First Urban Christians: The Social World of the Apostle Paul*. By Keith R. Bradley 1253
- LUCIANO PERELLI. *Il movimento popolare nell'ultimo secolo della Repubblica*. By Susan Treggiari 1254
- DAVID OWEN. *The Government of Victorian London, 1855-1889: The Metropolitan Board of Works, the Vestries, and the City Corporation*; KEN YOUNG and PATRICIA L. GARSIDE. *Metropolitan London: Politics and Urban Change, 1837-1981*. By Anthony Sutcliffe 1267
- GEORGE K. BEHLMER. *Child Abuse and Moral Reform in England, 1870-1908*. By Eric J. Evans 1268
- IAN BRITAIN. *Fabianism and Culture: A Study in British Socialism and the Arts, c. 1884-1918*. By Willard Wolfe 1268
- WOLFGANG MOCK. *Imperiale Herrschaft und nationales Interesse: "Constructive Imperialism" oder Freihandel in Grossbritannien vor dem Ersten Weltkrieg*. By Wilbur Devereux Jones 1269
- R. W. FERRIER. *The History of the British Petroleum Company*. Vol. 1, *The Developing Years, 1901-1932*; WILLIAM STIVERS. *Supremacy and Oil: Iraq, Turkey, and the Anglo-American World Order, 1918-1930*. By Briton C. Busch 1270
- J. A. CROSS. *Lord Swinton*. By Bentley B. Gilbert 1271
- PAUL BEW and HENRY PATTERSON. *Seán Lemass and the Making of Modern Ireland, 1945-66*. By Joseph M. Curran 1272
- ROBERT M. JENNINGS and ANDREW P. TROUT. *The Tontine: From the Reign of Louis XIV to the French Revolutionary Era*. By J. F. Bosher 1272
- LAURA S. STRUMINGHER. *What Were Little Girls and Boys Made of? Primary Education in Rural France, 1830-1880*. By Rachel G. Fuchs 1273
- WILLIAM H. SCHNEIDER. *An Empire for the Masses: The French Popular Image of Africa, 1870-1900*. By Kim Munholland 1274
- CHRISTIAN BAECHLER. *Le parti catholique alsacien, 1890-1939: Du Reichsland à la République jacobine*. By Alexander Sedgwick 1274
- JEAN-NOËL JEANNENEY and MONIQUE SAUVAGE, eds. *Télévision, nouvelle mémoire: Les magazines de grand reportage, 1959-1968*. By Steven Philip Kramer 1275
- JOHN EDWARDS. *Christian Córdoba: The City and Its Region in the Late Middle Ages*. By William D. Phillips, Jr. 1276
- JEROME R. MINTZ. *The Anarchists of Casas Viejas*. By Clara E. Lida 1276
- EDDY VAN CAUWENBERGHE. *Het vorstelijk domein en de overheidsfinanciën in de Nederlanden, 15de en 16de eeuw: Een kwantitatieve analyse van Vlaamse en Brabantse domeinrekeningen* [The Royal Domain and Government Finances in Fifteenth- and Sixteenth-Century Netherlands: A Quantitative Analysis of Flemish and Brabantine Domain Accounts]. By David Nicholas 1277
- RICHARD REITSMA. *Centrifugal and Centripetal Forces in the Early Dutch Republic: The States of Overijssel, 1566-1600*. By Sherrin Marshall Wytjtes 1278
- HANS CHR. SOLTER. *Adel og embede: Embedsfordeling og karrieremobilitet hos den dansk-norske adel, 1588-1660* [Nobility and Office: Distribution of Offices and Career Mobility among the Danish-Norwegian Nobility, 1588-1660]. By Heinz E. Ellersieck 1279
- TORKEL JANSSON. *Samhällsförändring och sammanslutningsformer: Det frivilliga föreningsväsendets uppkomst och spridning i Husby-Rekarne från omkring 1850 till 1930* [Transformation of Society and Forms of Organization: The Rise and Diffusion of Voluntary Associations in a Rural District, 1850-1930]. By Peter Vinten-Johansen 1279
- TOIVO NYGÅRD. *Suomalainen äärioikeisto maailmansotien välillä: Ideologiset juuret, järjestöllinen perusta ja toimintamuodot* [The Finnish Extreme Right between the World Wars:

MEDIEVAL

- DONALD WEINSTEIN and RUDOLPH M. BELL. *Saints and Society: The Two Worlds of Western Christendom, 1000-1700*. By Robert E. Lerner 1255
- ANTONIA GRANSDEN. *Historical Writing in England*. Vol. 2, *C. 1307 to the Early Sixteenth Century*. By Bernard S. Bachrach 1256
- A. R. BRIDBURY. *Medieval English Clothmaking: An Economic Survey*. By Robert L. Baker 1256
- HÉLÈNE MILLET. *Les chanoines du chapitre cathédral de Laon, 1272-1412*. By Joseph H. Lynch 1257
- JOACHIM BUMKE. *The Concept of Knighthood in the Middle Ages*. By John B. Freed 1258
- RANDOLPH STARN. *Contrary Commonwealth: The Theme of Exile in Medieval and Renaissance Italy*. By Julius Kirshner 1258
- JOHN M. NAJEMY. *Corporatism and Consensus in Florentine Electoral Politics, 1280-1400*. By Felix Gilbert 1259
- NORMAN GOLB and OMELJAN PRITSAK. *Khazarian Hebrew Documents of the Tenth Century*. By Horace W. Dewey 1260

MODERN EUROPE

- RALF DAHRENDORF. *On Britain*. By D. H. Aldcroft 1261
- BARBARA KANNER, ed. *The Women of England from Anglo-Saxon Times to the Present: Interpretive Bibliographical Essays*. By Donna T. Andrew 1262
- LINDA LEVY PECK. *Northampton: Patronage and Policy at the Court of James I*. By W. J. Jones 1262
- TIMOTHY GEORGE. *John Robinson and the English Separatist Tradition*. By Tai Liu 1263
- ROBERT W. MALCOLMSON. *Life and Labour in England, 1700-1780*. By Chris Fisher 1264
- BARRIE TRINDER. *The Making of the Industrial Landscape*. By Ian Donnachie 1264
- MARK NEUMAN. *The Speenhamland County: Poverty and the Poor Laws in Berkshire, 1782-1834*. By Anthony Brundage 1265
- RICHARD SHANNON. *Gladstone*. Vol. 1, 1809-1865. By Norman Gash 1266

Ideological Roots, Organizational Basis, and Forms of Activity]. By Marvin Rintala	1280	A. A. LEVANDOVSKII. <i>Iz istorii krizisa russkoi burzhuaznoliberal'noi istoriografi</i> : A. A. Kornilov [A History of the Crisis of Russian Bourgeois-Liberal Historiography: A. A. Kornilov]. By J. L. Black	1299
GORDON A. CRAIG. <i>The Germans</i> . By Jürgen Kocka	1281	G. P. KUROPATNIK. <i>Rossia i SShA: Ekonomicheskie, kul'turnye i diplomaticheskie svyazi, 1867–1881</i> [Russia and the USA: Economic, Cultural, and Diplomatic Ties, 1867–81]. By J. Dane Hartgrove	1300
GARY D. STARK and BEDE KARL LACKNER, eds. <i>Essays on Culture and Society in Modern Germany</i> . By Walter Struve	1282	IU. B. SOLOV'EV. <i>Samoderzhavie i dvorianstvo v 1902–1907 gg.</i> [Autocracy and the Nobility in 1902–07]. By John M. Thompson	1301
HUGO KUNOFF. <i>The Foundations of the German Academic Library</i> . By Paul R. Sweet	1283	NINA TUMARKIN. <i>Lenin Lives! The Lenin Cult in Soviet Russia</i> . By Robert H. McNeal	1301
HILDE HAIDER-PREGLER. <i>Des sittlichen Bürgers Abendschule: Bildungsanspruch und Bildungsauftrag des Berufstheaters im 18. Jahrhundert</i> . By Helen Liebel-Weckowicz	1284		
GUSTAVO CORNI. <i>Stato assoluto e società agraria in Prussia nell'età di Federico II</i> . By James Van Horn Melton	1285		
ROBERT EBEN SACKETT. <i>Popular Entertainment, Class, and Politics in Munich, 1900–1923</i> . By Vernon Lidtke	1285		
KARL ROHE, ed. <i>Die Westmächte und das Dritte Reich, 1933–1939: Klassische Grossmachtrivalität oder Kampf zwischen Demokratie und Diktatur?</i> By Edward W. Bennett	1286		
BERND WEGNER. <i>Hitlers Politische Soldaten: Die Waffen-SS, 1933–1945: Studien zu Leitbild, Struktur, und Funktion einer nationalsozialistischen Elite</i> . By James J. Weingartner	1287		
MARTIN VAN CREVELD. <i>Fighting Power: German and U.S. Army Performance, 1939–1945</i> . By Milan Hauner	1287		
MICHAEL WORTMANN. <i>Baldur von Schirach: Hitlers Jugendführer</i> . By Lawrence D. Walker	1288		
GERHARD BENECKE. <i>Maximilian I (1459–1519): An Analytical Biography</i> . By Paula Sutter Fichtner	1289		
RICHARD SCHÖBER. <i>Die Tiroler Frage auf der Friedenskonferenz von Saint Germain</i> . By Frederick Dumin	1290		
MELANIE A. SULLY. <i>Continuity and Change in Austrian Socialism: The Eternal Quest for the Third Way</i> . By Robert Schwarz	1290		
CARLO M. CIPOLLA. <i>The Monetary Policy of Fourteenth-Century Florence</i> . By Anthony Molho	1291		
JUDITH C. BROWN. <i>In the Shadow of Florence: Provincial Society in Renaissance Pescia</i> . By Melissa M. Bullard	1292		
CESARE MOZZARELLI. <i>Sovrano, società e amministrazione locale nella Lombardia teresiana, 1749–1758</i> . By Emiliana P. Noether	1293		
GIANNI TONIOLO, ed. <i>L'economia italiana, 1861–1940</i> . By Frank J. Coppa	1293		
MECHTHILD GOLCZEWSKI. <i>Der Balkan in deutschen und österreichischen Reise- und Erlebnisberichten, 1912–1918</i> . By Philip J. Adler	1294		
LAJOS LUKÁCS. <i>The Vatican and Hungary, 1846–1878: Reports and Correspondence on Hungary of the Apostolic Nuncios in Vienna</i> . By Bertam M. Gordon	1295		
GYULA TOKODY. <i>Deutschland und die ungarische Räterepublik</i> . By Thomas Spira	1295		
EILA HASENPELUG-ELZHOLZ. <i>Böhmen und die böhmischen Stände in der Zeit des beginnenden Zentralismus: Eine Strukturanalyse der böhmischen Adelsnation um die Mitte des 18. Jahrhunderts</i> . By Hanns Gross	1296		
SARAH MEIKLEJOHN TERRY. <i>Poland's Place in Europe: General Sikorski and the Origin of the Oder-Neisse Line, 1939–1943</i> . By Richard Woytak	1297		
CHARLES A. RU'D. <i>Fighting Words: Imperial Censorship and the Russian Press, 1804–1906</i> . By W. Bruce Lincoln	1298		
W. BRUCE LINCOLN. <i>In the Vanguard of Reform: Russia's Enlightened Bureaucrats, 1825–1861</i> . By Walter M. Pintner	1298		
		NEAR EAST	
		MEHDI KEYVANI. <i>Artisans and Guild Life in the Later Safavid Period: Contributions to the Social-Economic History of Persia</i> . By Roger M. Savory	1302
		ERVAND ABRAHAMIAN. <i>Iran between Two Revolutions</i> . By Gene R. Garthwaite	1303
		AFRICA	
		DOUGLAS PORCH. <i>The Conquest of Morocco</i> . By James J. Cooke	1304
		HAGGAI ERLICH. <i>The Struggle over Eritrea, 1962–1978: War and Revolution in the Horn of Africa</i> . By Christopher Clapham	1305
		JOAN VINCENT. <i>Teso in Transformation: The Political Economy of Peasant and Class in Eastern Africa</i> . By Randall M. Packard	1305
		ASIA AND THE EAST	
		GEORGE ALEXANDER LENSEN. <i>Balance of Intrigue: International Rivalry in Korea and Manchuria, 1884–1899</i> . By Frank W. Ikle	1306
		JAMES L. MCCLAIN. <i>Kanazawa: A Seventeenth-Century Japanese Castle Town</i> . By Susan B. Hanley	1307
		TAKAFUSA NAKAMURA. <i>The Postwar Japanese Economy: Its Development and Structure</i> . By Martin Bronfenbrenner	1308
		GIUSEPPE TUCCI. <i>The Religions of Tibet</i> . By Turtell V. Wylie	1309
		ALFRED W. MCCOY and ED. C. DE JESUS, eds. <i>Philippine Social History: Global Trade and Local Transformations</i> . By Theodore Friend	1310
		MICHAEL STENSON. <i>Class, Race, and Colonialism in West Malaysia: The Indian Case</i> . By Margaret Clark	1311
		R. DE BRUIN. <i>Islam en nationalisme in door Japan bezet Indonesië, 1942–1945</i> [Islam and Nationalism in Japanese-Occupied Indonesia, 1942–45]. By William H. Frederick	1312
		HARRY MORTON. <i>The Whale's Wake</i> . By Colin Newbury	1312
		GLYNN BARRATT. <i>Russophobia in New Zealand, 1838–1908</i> . By Roger C. Thompson	1313
		UNITED STATES	
		DAVID R. GOLDFIELD. <i>Cotton Fields and Skyscrapers: Southern City and Region, 1607–1980</i> . By Timothy J. Crimmins	1314

WARREN W. HASSLER, JR. <i>With Shield and Sword: American Military Affairs, Colonial Times to the Present.</i> By Harry L. Coles	1315	LEWIS GREEN. <i>The Boundary Hunters: Surveying the 141st Meridian and the Alaska Panhandle.</i> By Ted C. Hinckley	1333
JOHN PUTNAM DEMOS. <i>Entertaining Satan: Witchcraft and the Culture of Early New England.</i> By Stephen Nissenbaum	1316	GALEN CRANZ. <i>The Politics of Park Design: A History of Urban Parks in America.</i> By Dora P. Crouch	1334
CHARLES E. HAMBRICK-STOWE. <i>The Practice of Piety: Puritan Devotional Disciplines in Seventeenth-Century New England.</i> By J. F. Maclear	1317	STUART CREIGHTON MILLER. "Benevolent Assimilation": <i>The American Conquest of the Philippines, 1899-1903.</i> By Robert L. Beisner	1334
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MILTON E. FLOWER. <i>John Dickinson: Conservative Revolutionary.</i> By John A. Schutz	1321	MARGARET W. ROSSITER. <i>Women Scientists in America: Struggles and Strategies to 1940.</i> By Barbara J. Harris	1339
ROBERT L. HAMPEL. <i>Temperance and Prohibition in Massachusetts, 1813-1852.</i> By Ian Tyrrell	1321	C. L. SONNICHSEN. <i>Tucson: The Life and Times of an American City.</i> By John L. Kessel	1340
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EDWARD H. MADDEN and JAMES E. HAMILTON. <i>Freedom and Grace: The Life of Asa Mahan.</i> By Donald G. Jones	1323	JOHN D'EMILIO. <i>Sexual Politics, Sexual Communities: The Making of a Homosexual Minority in the United States, 1940-1970.</i> By Walter L. Williams	1341
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RALPH MANN. <i>After the Gold Rush: Society in Grass Valley and Nevada City, California, 1849-1870.</i> By Gunther Barth	1326	ROBERT H. FERRELL. <i>Harry S. Truman and the Modern American Presidency.</i> By Donald R. McCoy	1344
H. CRAIG MINER. <i>Wichita: The Early Years, 1865-80.</i> By Charles N. Glaab	1327	FRED I. GREENSTEIN. <i>The Hidden-Hand Presidency: Eisenhower as Leader;</i> JAMES C. DURAM. <i>A Moderate among Extremists: Dwight D. Eisenhower and the School Desegregation Crisis.</i> By Robert Griffith	1345
TED C. HINCKLEY. <i>Alaskan John G. Brady: Missionary, Businessman, Judge, and Governor, 1878-1918.</i> By James H. Ducker	1328	ROBERT A. CARO. <i>The Years of Lyndon Johnson. Vol. 1, The Path to Power.</i> By David Burner	1346
LESTER D. STEPHENS. <i>Joseph LeConte: Gentle Prophet of Evolution.</i> By Cynthia Eagle Russett	1328	MICHAEL TURNER. <i>The Vice President as Policy Maker: Rockefeller in the Ford White House.</i> By Bruce Miroff	1347
VALERIA GENNARO LERDA. <i>Il Populismo americano: Movimenti radicali di protesta agraria nella seconda meta' dell'800.</i> By Walter Nugent	1329		
JOHN L. THOMAS. <i>Alternative America: Henry George, Edward Bellamy, Henry Demarest Lloyd, and the Adversary Tradition.</i> By Henry F. May	1330		
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VERONICA E. VELARDE TILLER. <i>The Jicarilla Apache Tribe: A History, 1846-1970.</i> By Donald E. Worcester	1332		
GERALD P. FOGARTY. <i>The Vatican and the American Hierarchy from 1870 to 1965.</i> By George Q. Flynn	1333		

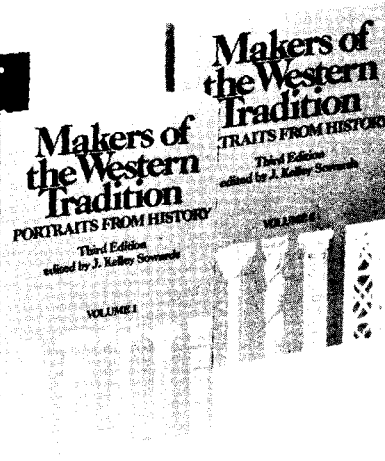
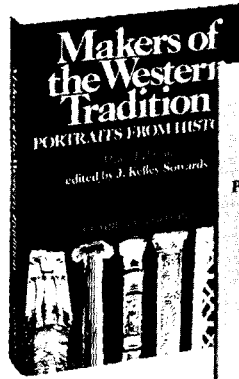
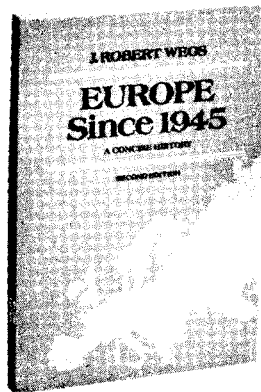
CANADA

NOËL BAILLARGEON. <i>Le Séminaire de Québec de 1760 à 1800.</i> By Peter N. Moogk	1348
GREGORY S. KEALEY and BRYAN D. PALMER. <i>Dreaming of What Might Be: The Knights of Labor in Ontario, 1880-1900.</i> By Michael S. Cross	1349

LATIN AMERICA

NICHOLAS P. CUSHNER. <i>Farm and Factory: The Jesuits and the Development of Agrarian Capitalism in Colonial Quito, 1600-1767.</i> By Eric Van Young	1350
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IAN JACOBS. <i>Ranchero Revolt: The Mexican Revolution in Guerrero</i> . By William H. Beezley	1351	GERHARD JACOB-WENDLER. <i>Deutsche Elektroindustrie in Lateinamerika: Siemens und AEG, 1890–1914</i> . By Warren Schiff	1352
MICHAEL MONTEÓN. <i>Chile in the Nitrate Era: The Evolution of Economic Dependence, 1880–1930</i> ; THOMAS F. O'BRIEN. <i>The Nitrate Industry and Chile's Crucial Transition, 1870–1891</i> . By Thomas C. Wright	1351	MARK FALCOFF and FREDERICK B. PIKE, eds. <i>The Spanish Civil War, 1936–39: American Hemispheric Perspectives</i> . By Thomas J. Knight	1353
Collected Essays	1355	Annual Index	1366
Documents and Bibliographies	1360	Index of Advertisers	38(a)
Other Books Received	1362		



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God's Controversy with Jacobean England

MICHAEL MCGIFFERT

It is the sins of the people that bringeth every commonwealth to ruin. Everyone will say as much, but yet in our practice we hold an other strange axiom that goeth for current amongst us: it is the sins of the people that upholdeth every commonwealth.

—Lancelot Dawes, 1609

Well, there must be an end of sinning or else God will make an end of us.

—Robert Gray, 1609¹

JOHN DOWNNAME, RECTOR OF ST. MARGARET'S, LOTHBURY, in London, greeted the seventeenth century with two books that broke ground in English practical divinity. *The Christian Warfare* (1604), a manifesto of militant piety, won a position at the head of a distinguished line of spiritual enchiridia, and historians have recognized its role in the rise of puritanism.² Much less well known, but central to the present purpose, is *Lectures upon the Four First Chapters of the Prophecy of Hosea* (1608), showing how God, in the catchphrase of Downname's text, had "a controversy with the inhabitants of the land." Wedding learning to zeal, this treatise was the first major Protestant commentary on Hosea and one of the largest works of Old Testament exegesis by an Englishman to date. It marked a coming of age in English biblical scholarship—the kind of scholarship that did not repose in the cloister of the schools but made one great school of all the people of the realm.

The homiletic theme of God's controversy with England had risen with the Reformation, and fifty years of preachers since John Aylmer had (while acknowledging the transnational fraternity of Protestant communions) affirmed that God was somehow signally English—but it was left to Downname to show exactly in what sense God and England had claims upon each other. Therein lies the value of his treatise for the historian who is satisfied that something more may be said about the

The research on which this article draws was materially assisted by the National Endowment for the Humanities, the College of William and Mary, and the Institute for Early American History and Culture.

¹ Dawes, *God's Mercies and Jerusalem's Miseries* . . . (London, 1609), h7r; and Gray, *An Alarum to England, Sounding the Most Fearful and Terrible Example of God's Vengeance That Ever Was Inflicted in This World upon Mankind for Sin* (London, 1609), 18v. Spelling and punctuation in the titles and quotations have been modernized throughout.

² William Haller, *The Rise of Puritanism: or, The Way to the New Jerusalem as Set Forth in Pulpit and Press from Thomas Cartwright to John Lilburne and John Milton, 1570–1643* (New York, 1938), 92, 155–58. Haller's *Rise of Puritanism* has as its frontispiece the title page from Downname's work (1634 edn.).

ideological origins of the English Revolution. Downname's *Hosea* fitted into the genre of the national morality sermon but went far beyond any of its predecessors in giving system, substance, and force to the form. Above all, it joined the example of Israel—the pattern for God's relations with England—with the doctrine of the two covenants.³ Downname may accordingly be said to have invented the normative Jacobean version of what historians have called the Jeremiad but what, in recognition of his source and achievement, will here be named the Hosead. In its day—a day of comparative serenity in public affairs—the Hosead expressed the broad reforming mind of the church; it was ecclesiastically unitive, and, although it promoted reform, it did not dream of revolution. At the same time, with an eye to the future, we may discover in Downname's explanation of the divine anglophilia—his contribution of scriptural precision and doctrinal cogency to the religious ideology of nationalism—a set of discriminations that dotted a line along which both church and nation later divided.

AS SOCIAL CRITIC, Downname said nothing that had not been said a hundred or a thousand times before. Elizabethan England had never lacked self-nominated prophets for whom the times were bad and growing worse. They typically recited England's blessings, offenses, and woes, scaled by the preacher's preference and the promptings of his text. Some were presbyterians who made church reform prerequisite to the general reformation of morals and manners. Others were sound sons of the Elizabethan religious settlement, for whom presbyterians were part of the problem. Most may be termed puritanical in the conventional moral meaning of the word, but in this regard the label tends to lose discriminative value. What the record tells us is that reformers and conformists shared a conviction of England's special status, exhibited anxiety for England's welfare, and wished their country to be godly and good. Their function, in retrospect, was to forge a morality of nationhood. It was their premise, and at the same time their point to prove, that "God hath placed us Englishmen here in one commonwealth, also in one church, as in one ship together."⁴

These clerical monitors took cues from the example of Israel, God's typical chosen people, nation, and church. By Downname's time, the Judaic characterizing of God's Englishness, and of England's prominent place under divine watch and ward, had achieved the power of a paradigm. It was a commonplace of commonplaces—a simple matter of fact—that the Deity's "great mercies towards us Englishmen above many other nations make his judgments more heavy" because "we are like unto the children of Israel"; that England enjoyed God's favor but had no guarantee of keeping it, because the people, like "the old Israelites," had "rewarded the Lord evil for good"; that, although God had "tied himself to this whole nation," the nation had broken faith with him, so that he had with it the same

³ See Michael McGiffert, "Grace and Works: The Rise and Division of Covenant Divinity in Elizabethan Puritanism," *Harvard Theological Review*, 75 (1982): 463–502.

⁴ Thomas Tymme, *A Preparation against the Prognosticated Dangers of This Year, 1588* (London, 1588), B7r. Surprisingly, we do not have in print a full study of the preaching of the Israelite paradigm.

controversy he had prosecuted against Israel.⁵ Illustrations are legion: it was a Protestant and patriotic certainty that England's history presented the best modern record of God's providence and that Albion was his *pied à terre*.

Israel meant much for England because the experience of the Jews made available the rich homiletic resources of the Old Testament. For this the preachers had the warrant of Paul, whose letter to the Romans provided the platform for the Israelite paradigm. Israel, Paul wrote, had the adoption, the glory, the covenants, the law, the service of God, and the promises—every spiritual benefit and privilege imaginable. Born Satan's slaves like the rest of mankind, the Jews had been emancipated by adoption as God's sons. Their glory was the glory of his presence, symbolized by the Ark of the Covenant. They had the tables or covenants of the law for rule, guide, and stay. God's kingliest promises of grace and favor engaged their service.⁶ In addition, they dwelt (most of the time) in a sweet land where they enjoyed (some of the time) the sway of good kings and received (when needed) the therapeutic attentions of holy prophets. Nowhere in the literature or history of the world could England's preachers have found a model better suited to their use and need. On this base they built the towering scaffold of moral nationalism.

It took no theological profundity to make or grasp the argument from the paradigm. England in this simple simile was like Israel in being God's most favored nation, in superiority of spiritual and temporal goods, and accordingly in magnitude of debt. The mode of payment of the debt was set by the Judaic moral law: the paradigm enjoined a rule-based morality and a law-abiding piety. Shortcomings would be penalized by afflictions proportioned to the default and dealt to the nation generally. The ultimate sanction was the doom of Lo-Ammi—*not my people*. To stay in God's graces, the nation must be sent to the school of the law to learn the three R's of practical divinity: repent, reform, return.

From the simile flowed an equally simple syllogism: God dealt so with Israel; we are like Israel; God does and will deal so, or very like, with us. The preachers easily proved the minor premise. Looking back, they could cite a dazzling run of deliverances, starring 1558, 1588, and 1605. England had been brought out of worse than Egyptian darkness; its Deborah had set up the true faith; the church, whatever its vestigial defects, approximated the model of the apostles. Looking about them and taking temporal inventory, the preachers saw a land surpassing Canaan, filled with a happy, hardy, prosperous, and prolific people, guarded and governed by godly magistrates. This people's spiritual advantages could be attributed only to divine favoritism. Looking abroad, scanning the world, the ministers could casually dismiss the claims of nations that might crowd forward as rivals in God's court. Israel had been nonpareil, and where could be found a country with half England's qualifications? God's people had to be Protestant, but the Swiss Protestant cantons were too small, the German states too small and Lutheran, the Scandinavian lands too Lutheran and remote, Holland too hospitable to religious extravagance. Incontestably, England possessed the lion's share of

⁵ John Stockwood, *A Sermon Preached at Paul's Cross . . . 1578* (London [1578]), 20, 22; John Carpenter, *Time Complaining . . .* (London [1582]), B1r, A5r; and *The Reformation of Religion by Josiah* (n.p. [1590]), A2r.

⁶ Romans 9:4–5, with the Geneva glosses.

holiness and happiness. This comparative argument was finally clinched by the successes of Catholic arms in the Thirty Years' War, when Protestant Bohemia and the Palatinate fell to Antichrist. Plainly, if God did not mean to leave the world without a holy land, it could be none save England.

This much proved, it remained to show that England, like Israel, was not half as holy as it ought to be but in fact was long on sin and short on shame. God had been more than gracious, yet England's wickedness matched Israel's on both tables of the law. The nation had godly magistrates and painful ministers but heeded neither, and the best that could be said of these wardens of commonwealth and church was that they were more or less unequal to their duty. Consequently, evil-doing besmirched the public character. In these modern times as in the Middle Ages, sin was no respecter of rank: every class and profession bore a share of the national guilt; if the country was bad, the city was worse, and the court worst of all. Slips and falls of individuals might evoke tender pastoral concern, but vice in bulk—the gross, general, insufferably common vice of the nation en masse—struck prophetic fire in men who cast themselves according to the biblical repertory as God's ambassadors, watchmen, heralds, witnesses, messengers, cryers. They could grieve for victims of dearth and plague, but as God's alarmists they were bound to justify his ways of warning and punishing a vicious folk. Finally, the preachers wrapped up their argument in a typological exegesis featuring the scriptural sequence of adoption, liberation, constitution as a moral nation, rebellion, and retribution, whereby "Israel and England, though they lie in a diverse climate, may be said right parallels, not so unfit in cosmological as in theological comparison."⁷

It was at all times the empirical evidence that made the parallels seem right. When the record was explained, it became clear beyond cavil that God was especially English. The past thus served the preachers, and they in turn, while working up the paradigm, bestowed on England a national character. Foxe had drawn a part of the plan, but the record was not one that could be run up in a day or a decade, blaze martyrs as they might.⁸ Time was needed to fill it out and give it force. Given time—given, that is, a reasonably regular routine of stiffening adversities, with remissions spaced to make the pain instructive, not depressive—the paradigm would tighten its hold on the religious mind. Each drought or plague proved it—because where God loved, he chastised—and each respite doubled the proof. If England reformed under the rod, well and good: God then called off the angel of death. But in fact England never did reform; therefore, each reprieve figured forth his mercy toward his people—and put the nation's debt still further in arrears. The paradigm thus not only confirmed but also aggrandized itself. To the degree that it ruled the interpretation of events, events would cumulatively document its truth; and with each turn of the screw the stakes rose and the odds against England lengthened. There was more in this than the Bellman's principle of repetition, and much more than mere clerical bugling. Whether England was going to the devil in an objective sense need not be our question; the point is that the

⁷ Thomas Adams, *England's Sickness Comparatively Conferred with Israel's* (London, 1615), 1. It was a good line: John Jones lifted it almost verbatim for his *London's Looking Back to Jerusalem . . .* (London, 1633), 26.

⁸ For Foxe's contribution to English nationalism, see William Haller, *The Elect Nation: The Meaning and Relevance of Foxe's Book of Martyrs* (New York, 1963). Foxe, however, did little with the Israelite paradigm, and the paradigm preachers made only desultory use of his work, mainly for anecdote.

paradigm impelled its promoters not only to perceive steady declension but to pry out every iota of evidence that would make God's case stick.⁹ Over the long haul, the logic of the paradigm forced escalation of its terms and pushed England ever nearer to the crisis of which the paradigm was the self-fulfilling prophecy.

ALTHOUGH BY 1600 THE EXAMPLE OF ISRAEL had gained currency in the pulpit, it was not, in fact, the only one available in Scripture or even the most pertinent of patterns. The leading alternative—potentially a competitive model—portrayed England not as Israel but rather as Assyria, the imperial state of the gentiles. When God set his sights on this splendid prize, he sent the prophet Jonah to order Ninevah to repent in forty days or take the consequences. Jonah jibbed but did the job in the end, and with only three days' preaching (according to the Genevan headnote, although some thought he needed only a single sermon) he brought to their knees the king and, by the king's command, the entire people. England, though much inferior to Assyria in grandeur, was certainly presentable as a missionary field for the calling of the gentiles, and Elizabethan clergymen had used the tale to dilate on the submission of ministers to God and subjects to kings, on the need to wipe out Catholicism in England, and on the urgency of national repentance and conversion. Jonah was thus a fitting text—more so, one might have thought, than prophecies addressed to Jewry—and it had superior typological credentials; for Jonah was a signal type of Christ (his in-and-out experience with the whale prefigured Christ's death and resurrection), and Christ had declared a decided preference for "the Ninevites before the Israelites because the Ninevites repented at the one only sermon of Jonas," whereas "the Israelites would not be converted unto the Lord by many sermons." It followed that, if God did not intend "to save the realm of England, he would not have sent Jonas (I mean the preachers of his word) unto the same."¹⁰

This text was taken up near the close of Elizabeth's reign by churchmen thoroughly loyal to the established church. Lecturing on Jonah at York in 1594, John King begged God to "say of England even forever and ever, . . . *Here will I dwell; I have chosen England for my habitation.*" The Deity would come to stay, King explained, if England would rebuff the presbyterians' "hasty, headlong, heathenish endeavors to reform a church, to dissolve government, to disjoint order, to compel a prince." These sermons received seven editions to 1618; King rose to the see of London. In the same decade George Abbot preached Jonah for five years at Oxford, making unmistakable his interest in the peace and unity of the church of

⁹ Only a few bright bubbles rose through this murk (if the printed record is representative). In 1618, for instance, Thomas Gainsford declared England "nearer the example of Canaan's happiness than any other nation," with merely a glance at "national vices" and "customary sins"; Gainsford, *The Glory of England* (London, 1618), 242, 162. Five years later G. Warburton congratulated the king on reigning over a land that was "for strength as another tower of David, for pleasure as another Eden, and for profit as another Canaan"; Warburton, *King Melchizedech* (London, 1623), 40. Also see Samuel Purchas, *The King's Tower and Triumphant Arch of London* (London, 1623). Men of the sunny side of the paradigm were already in the frying pan by 1623.

¹⁰ Thomas Tymme, *News from Ninevah to England, Brought by the Prophet Jonas* (London, 1570), A5r, A5v. Tymme presented this work as a translation from the Latin commentary by Joannes Brenz; but the title was his own, and he interjected the references to England. For Christ's preference, see Matthew 12:38–41 and Luke 11:29–32.

which he was to be archbishop. At Paul's Cross in 1602 Robert Wakeman, fresh from Oxford, addressed Jonah "to the people of England in general" in an effusion of moral fervor, patriotic sentiment, and churchly zeal.¹¹ Wakeman scourged sinners like any puritan but harbored a fear of nonconformity that blazed out in a later attack on "the scum of the commons, . . . the fiery, furious, brainsick, impure Puritan, . . . in their attempts pragmatistical, in their humors fantastical, in their looks hypocritical, in their profession pharisaical, in their opinions anabaptistical, in their doctrines schismatical, in their words angelical, in their deeds diabolical."¹² From such evidence one might infer a battle of paradigms pitching conformity against nonconformity: Jonah against Jeremiah, Ninevah set off typologically against Jerusalem, England as the Assyria of the gentiles rather than the Canaan of the Jews—in a word, Anglican versus puritan. But the documentation cannot be pressed that far, for preachers were not fussy in their choice of prophets but used their Old Testament sources interchangeably. Wakeman, for example, took his text in 1602 from Jonah but most of his substance from prophets who spoke to Israel. Moreover, it is difficult to discover any deep affinity between Jonah and the prelatical mind of the period, save perhaps on the point of clerical compliance. In any case, the Israelite paradigm was by no means monopolized by dissenters but had long since become the common property of the church.

Judging from the printed remains, Jonah had only a short run; the notion of gentile England never achieved paradigmatic status.¹³ We can speculate on the reasons. Biblically, the Israelite ore was immeasurably richer in sins, sanctions, and graces. Symbolically, England was insufficiently imperial. Personally, as an exemplary figure apart from his efficiency in Ninevah, Jonah was not very satisfactory; in the first instance he balked, and in the last he got starchy about his reputation because God had not carried out the threat to destroy Ninevah. Professionally, the swift success of Jonah's mission gave little scope to ministers whose purpose, in line with Paul's teaching of the law, was to increase the sight and sense of sin and whose calling (one may suggest) depended on sinners resisting and sin persisting. Theologically and pastorally, the abruptness of Ninevah's conversion violated the ministers' experience of the hardness of the thing. This same deficiency goes far to explain why Jonah did not—indeed, could not—become a commanding model for the national morality sermon. God's public proceedings with nations, like his

¹¹ King, *Lectures upon Jonas* . . . (Oxford, 1597; reprint edn., London, 1618), 21, 199; Abbot, *An Exposition upon the Prophet Jonah* . . . (London, 1600); and Wakeman, *Jonah's Sermon and Ninevah's Repentance* . . . (2d impression, Oxford, 1606), 55.

¹² Wakeman, *The True Professor Opposed against the Formal Hypocrites of These Times: A Sermon Preached at Saltash in Cornwall, July 15, 1620* (London, 1620), 25–27. Wakeman's preface indicates that he had been choking down this bile for "many years"; *ibid.*, A3v. For a contemporary puritan use of Jonah, see Henry Smith, *Two Sermons of Jonah's Punishment* (corr. edn., London, 1602). Smith warned that, even if God should destroy London as he had Sodom, Ninevah, and Jerusalem, he would have a saving care for his true worshippers, because "his covenant doth always provide for his chosen, although they be but a remnant"; *ibid.*, c6r.

¹³ John Verneuil, who listed English commentaries in Bodley's collection, noted for the later period only a single sermon by William Attersoll, preached ca. 1627; Verneuil, *Nomenclator* (Oxford, 1637). We might add H. S., *Jonah the Messenger of Ninevah's Repentance* (London, 1637), but this work deals less with Ninevah than with Jonah himself. The book of Jonah, it seems, made better matter for preaching in the earlier stages of England's conversion, as in 1550 when John Hooper expounded it in a series of Lenten sermons to young Edward VI (a bishopric soon followed) Hooper's *An Oversight and Deliberation upon the Holy Prophet Jonas* (an early example of the national morality sermon, fitted to the youth of both the king and English Protestantism; see Hooper, *Early Writings*, ed. Samuel Carr, Parker Society Publications, 20 (Cambridge, 1843), 431–558).

private ones with souls, required, after all, a long drawn-out succession of provocations, warnings, afflictions, recoveries, and such, all of which made up the meaning of moral existence. But Ninevah had hardly time for either misery or mercy; the trial was too short and the result too sweet to supply the kind of self-reinforcing drive that powered the paradigm of Israel.

ALTHOUGH BY DOWNNAME'S TIME THE JUDAIC MODEL was becoming standard for England, the logic of its applicability remained implicit. Only rarely, and never rigorously, did the preachers raise the question, In what sense, exactly, could England be said to be God's particular nation, people, and church? Their mode was practical; their temper, empirical; their method, descriptive and declarative. They apparently felt no call to explain how God's dealings with ancient Jewry typified his dealings with England. Rather, they took the paradigm for granted, never rationalized it, never felt impelled to defend it or even to examine its grounds. They show no sense of transforming a metaphor into a myth. There were simply two historic truths—one old and Jewish; one new, Christian, and especially English—that in combination demonstrated the continuity of the *Heilsgeschichte* and the consistency of Providence. Bearing only the lightest intellectual ballast, the paradigm ascended on the wings of national spirit and clerical distress.

The deficiency, if such it was, was rectified during the first several years of James's reign, years that brought an unprecedented surge of clerical anguish over England's sins. As the flood of tears and ink rose, the terms of the paradigm were perfected. This was a matter of shaping its elements into an argument while setting the argument on a theological base. The first could not be done without the second; John Downname led in doing both. The upshot was the *Hosead*, into which Downname built the first systematic definition of the Israelite paradigm. He compiled the data of his land's relations with God, explained the record in terms drawn from the latest advance of the covenant divinity of his day, and fitted it to his text as proof of God's controversy with Jacobean England.

Downname (1571?–1652) was born into a generation of churchmen that historians have found easy to neglect, in part because it flourished between the livelier, louder doings of its fathers and sons and had a milder character than theirs. He, too, though long lived, seems to have been soon dismissed from memory. Thomas Fuller noticed him perfunctorily as “a painful and profitable preacher in London”—one of a great many such—and cut several years off his life by having him die in 1644. Samuel Clark, the puritan hagiographer, did not notice him at all. Edward Leigh left him off his 1656 honor roll of more than a century of learned writers. And William Crowe entered *Hosea* in his post-Restoration catalogue of English exegeses but ascribed it to Downname's older brother George.¹⁴ All this may be illusory. Still the impression grows that Downname has been accorded less than ordinary recognition for his services to his church and his contributions to the rising spirit of puritanism. The explanations are possibly that he was at heart a

¹⁴ Fuller, *The History of the Worthies of England* (London, 1662), 1st pagination, 191 (misnumbered 291); Leigh, *A Treatise of Religion and Learning* (London, 1656); and Crowe *The Catalogue of Our English Writers on the Old and New Testament* (1663; 2d edn., London, 1668), 120. Downname's niche in Leigh's *Treatise* would have been on page 182.

conformist, not a controversialist; a pen-man, not a pulpiteer or politician; a steady, modest, even a diffident soul, happiest with his head in a book; in the church, a good man for a committee; in his study, a laborious scholar, smelling of the lamp, whose sufficient learning was dusted with pedantry and who, in his dutiful and decent way, was something of a hack. Such a man could solve the problem of the paradigm and never, perhaps, know what he had done; certainly he was not likely to advertise it.

Son of a bishop (Chester) and brother of another (Derry), he was trained at Christ's College, Cambridge, and held several London pulpits through a career of over half a century. Contemporaries knew him best as the Tuesday lecturer for thirty years at St. Bartholomew Exchange. Edifying tracts flowed from his pen. His *Christian Warfare* went through four editions in twenty-four years, not counting the three parts added to it, 1611–18, under other titles. He edited Henry Finch's *The Sum of Sacred Divinity*, Thomas Sutton's lectures on Romans, and James Ussher's *A Body of Divinity*. He digested Clement Cotton's large official concordance into the short one that was printed at the back of the King James Bible until at least the end of the eighteenth century. From an early date he formed close connections among London's moderate ministers. For many years he advised the Haberdashers' Company on church patronage; in the 1640s he served as parliamentary licenser for the press and helped prepare a set of annotations of Scripture intended to supplant the Genevan marginalia. He was, in sum, one of the central men, and, when we call him a puritan, we recognize not a factional agitator of an Elizabethan type but a moderate who walked the church's middle aisle—until he found it leading to Laud's altar.¹⁵

Downname succeeded his brother George at St. Margaret's in 1602 and through George became associated, as a protégé of sorts, with Calvinist churchmen such as James Montague, to whom he dedicated *Hosea* in the same year that George (prompted by Archbishop Richard Bancroft) affirmed the divine right of bishops at Montague's consecration to Bath and Wells. He kept his place through Bancroft's dusting of the pulpits after the Hampton Court Conference. His substantial conformity was never in question. He had no brief against bishops. He judged the church a true church, even while it retained some "relics of idolatry," held that the voiding of such relics was business not for "private men" but for the civil magistracy, and advised junior ministers to "undergo some rites and ceremonies, though very inconvenient [in conscience], if it cannot be proved that they are in their use simply evil and unlawful," for there could be "no inconvenience so great as the neglect of preaching Christ crucified."¹⁶ These were the sentiments of puritans who took their

¹⁵ Biographical information is exasperatingly fugitive and fragmentary. See *Dictionary of National Biography*, s.v. "Downham, George," and "Downham, John." Details of Downname's London career and connections are brought to light in Paul Seaver, *The Puritan Lectureships: The Politics of Religious Dissent, 1560–1662* (Stanford, 1970), esp. 158, 175, 199. Seaver's reconstruction of the London brethren is illuminating; *ibid.*, esp. 273–74, 325–26, 362. Wilfred R. Prest solved the bibliographic puzzle of Finch's work in "The Art of Law and the Law of God: Sir Henry Finch (1558–1625)," in Donald Pennington and Keith Thomas, eds., *Puritans and Revolutionaries: Essays in Seventeenth-Century History Presented to Christopher Hill* (Oxford, 1978), 106–07.

¹⁶ Downname, *Lectures upon the Four First Chapters of the Prophecy of Hosea* (London, 1608), 1st pagination, 274–75. Downname swam in the "mainstream of Calvinist episcopalianism" described by N. R. N. Tyacke; see Tyacke, "Puritanism, Arminianism, and Counter-Revolution," in Conrad Russell, ed., *The Origins of the English*

lead from mentors like Perkins, Richard Greenham, or Laurence Chaderton and who found a politic friend at court in John Preston. If Downname's practice amounted to only two cheers for the status quo, conformity for him was no paper façade but reflected his readiness to give ecclesiastical authority the benefit of almost every doubt.

Of his reasons for writing on Hosea, he told his readers only that he meant to fill a gap in England's bookshelf: although there were already so many religious works in print that "a man may spend his whole time in reading titles and inscriptions," only a very few "sound expositions" of Scripture were available in English, and none at all of Hosea.¹⁷ But this explanation will not travel far, for the same could have been said of other prophets, including the major four—Isaiah, Jeremiah, Ezekiel, and Daniel.¹⁸ Filling Downname's silence with speculation, we may venture reasons for his choice from evidence of fit between the man, the times, and the text. These were years when ministers like Downname made peace with the church by making war on the world. They had no mind to follow the Elizabethan dissidents in politicking and polemics. By letting go of Cartwright's cause, they freed their hands for other work. Theirs was the day, in Joseph Mede's taxonomy, neither of the "ecclesiastical puritan" of the past nor of the "political puritan" of the future, who by the early 1620s began to raise questions about the "liberties of the people" and "prerogatives of sovereigns," but of the "moral puritan," whose present labors to reform the people earned from Mede the epithet "vulgar."¹⁹

Hosea's charges against "the inhabitants of the land" chimed with this reoriented puritanism, for reformers could handle the topic of God's controversy without being burned by it. The prophet's target, at least in the four chapters that Downname worked through, was so general as to be unexceptionable—namely, the sins of the entire people. In assailing common crimes and vices the English Hosea not only generalized the indictment but aligned himself with the forces of temporal law and order. Even as he chided magistrates for laxity, he demonstrated full and genuine assent to their authority. It was no insult for a sheriff or a justice of the peace to be told from the pulpit (unless by a young hothead) that, "of all the enemies of a commonwealth, none is so great as remiss, careless, and corrupt magistrates, for they are a cause of God's general judgments, whenas their severity would prevent."²⁰ Such preaching ritually cemented the bond between pulpit and bench. Furthermore, while Downname criticized the functioning of civil and

Civil War (London, 1973), 129, and "Arminianism in England in Religion and Politics from 1604 to 1640" (D.Phil. thesis, Oxford University, 1969). Also see Patrick Collinson, *The Religion of Protestants: The Church in English Society, 1559–1625* (Oxford, 1982); and Peter Lake, *Moderate Puritanism and the Elizabethan Church* (Cambridge, 1982).

¹⁷ Downname, *Hosea*, a6r.

¹⁸ An exception is George Joy's *The Exposition of Daniel the Prophet* (Geneva, 1545; London, 1550), a compilation from Melancthon, Oecolampadius, Pellican, and Draconites. Fairly substantial treatises or sermon sets were available in English for each of the minor prophets Joel, Obadiah, Jonah, Micah, and Haggai, and Lambertus Danaeus covered the whole group of them in *A Fruitful Commentary upon the Twelve Small Prophets* (1585), trans. John Stockwood (London, 1594).

¹⁹ Mede to Sir Martin Stuteville, April 14, 1623, British Library, Harleian MSS 389, f. 314, quoted in Kenneth Shippo, "The 'Political Puritan,'" *Church History*, 45 (1976): 196.

²⁰ Richard Stock, *A Learned and Useful Commentary upon the Whole Prophecy of Malachi* (ca. 1608), ed. Samuel Torshell (London, 1641), 96.

religious institutions, he had no quarrel with their forms and, therefore, no reformation to promote. Rather, he put a legalistic gloss on God's controversy, constraining it into "a judicial form of proceeding" in the "court of conscience."²¹ Such was the tack and temper of centrist churchmen during the interlude between the collapse of political puritanism in the 1590s and its revival in the 1620s. For a generation, while the center held, moderates like Downname could work in conscience as well as prudence to bind the people to constituted authority under the rule of laws that still seemed essentially consonant with the law of God.

Text and times appear also to have melded to make the Hosea a specifically Jacobean phase in the evolution of the paradigm. Although the evidence is very slight and differences between prophets lay chiefly in the eye of the commentator, Hosea seems intermediate between Jonah and Jeremiah when preachers measured out the national cup of joy or woe. In the 1590s Jonah's success story buoyed hopes by proving "the propensity of God's nature to show mercy."²² The prophecy of Jeremiah, by contrast, lent itself to lamentation; ministers needing a text for a plague sermon turned readily to Jeremiah or to Amos. During the pestilence of 1604 one pastor called it Jeremiah's "chiefest point" that "now no hope of pardon was left unto the Jews. They had so long despised mercy that now vengeance was come"; accordingly, Jeremiah did not admonish, exhort, or even pray, but only mourned. Another preacher in a trying time twenty years later read Amos to the effect that "the Israelites must die. Justice must have her course; mercy will do no good. Yet it grieves his soul to think that they must die."²³ More grim than Jonah but less glum than Jeremiah or Amos, England's Hoseas kept God's sentence pending. They did not expect to match Jonah's exploit: "One Jonah converted Ninevah. What a shame is it to us that so many Jonahs should do no good in London?"²⁴ Yet neither were they ready to compose a threnody for their lovely land. Doomful prophets like Amos and Jeremiah rose on the tide of troubles in the 1620s and 1630s. By contrast, Hosea seems suited to the middle years of James, with the nation at peace abroad, the plague largely in abeyance, the puritan fire-brand burned out or put out, the Catholic menace mostly scotched, the Spanish match still in the egg, Laud still in the wings.²⁵

DOWNNAME MEANT TO DO THE WHOLE BOOK, but his printer "overruled" him and took away the manuscript when only four of fourteen chapters were finished (enough to make a plump quarto of over 650 pages). Stopping where it did, the commentary climaxes in God's controversy, for which the *locus sanctus* is the first three verses of

²¹ Downname, *Hosea*, 2d pagination, 49, 54. Downname's sense of the controversy was very different from that of the anonymous author of *The Reformation of Religion by Josiah* (1590), who declared that God had "tied himself to this whole nation," demanded to know whether the nation had "kept covenant," and, when answered in the negative, hurled Hosea 4:1; *ibid.*, A2r-v. This tract, a late shot in the presbyterians' failed campaign, was fired again in 1641 to signal the revival of the cause.

²² B. P., *The Prentice's Practice in Godliness, and His True Freedom* (London, 1608), 55r.

²³ Thomas Pulein, *Jeremiah's Tears* (London, 1608), B3v-4r; and William Worship, *Three Sermons Preached at Three Successive Visitations at Boston in the County of Lincoln* (London, 1625), 16.

²⁴ B. P., *The Prentice's Practice in Godliness*, 35r.

²⁵ This is not to overlook, inter alia, the advances of puritan sabbatarianism that led James to issue the Book of Sports, the actions of authorities against recusants and dissenters, the preliminary stirrings of ceremonial Arminianism at Gloucester during Laud's deanship, or the doctrinal crystallization of the dispute over

the fourth chapter. Downname quoted the passage (varying only a preposition) from the Geneva translation:

Hear the word of the Lord, ye children of Israel, for the Lord hath a controversy with the inhabitants of the land because there is no truth nor mercy nor knowledge of God in the land.

By swearing and lying and killing and stealing and whoring they break out, and blood toucheth blood.

Therefore shall the land mourn, and everyone that dwelleth therein shall be cut off, with the beasts of the field, and with the fowls of the heaven, and also the fishes of the sea shall be taken away.

Downname's view of the controversy as "a judicial form of proceeding" conforms to the traditional theme of the prophetic lawsuit for which Hosea struck a keynote. Continental Protestants had published over twenty expositions of Hosea (though none as robust as Downname's); most of these made a court case of Hosea 4:1–3. We do not know how many Downname had read, but we learn from his marginalia that he knew the work of Joannes Drusius as well as that of the medieval Jewish scholar David Kimchi, the latter probably in the form of Latin excerpts in Jean Mercier's composite commentary on the minor prophets published in 1583.²⁶ Downname's adoption of the motif is not surprising; what compels interest is his dramatic exploitation of it. Where his forerunners had mostly been content to denote the Latin equivalents of "controversy"—*disceptatio*, *contentio*, *iudicium*, *lis*—Downname embroidered the trope with wholly unprecedented virtuosity. He thus set an imitable example for clerical reformers who later expanded the charge that "the people . . . of this land are in most heinous manner guilty of all these sins whereof the Israelites are here convicted and condemned."²⁷ The difference between Downname's precursors and successors measures the leap from school to pulpit—from a scholastic to a homiletic mode of address—and also, apparently, from the Continent to England, since the preachers of no other nation pled God's suit with the prosecutorial zeal of the English Hoseas.

Downname milked the metaphor. The people "are summoned to appear and [are] arraigned before the tribunal of God's judgment" to answer for "their capital crimes" charged in several "bills of indictment." God takes the triple role of plaintiff, judge, and executioner: he brings the complaint, sifts the evidence, passes sentence, and will ultimately punish. In his Star Chamber the people appear collectively as defendant; the nation stands trial as one, because there has been "a general defection," and it will have one general judgment, because "God punisheth national sins with national punishments." No question of the justice of this arrangement

Arminianism that resulted, in part, from James's intervention in the Vorstius affair and the Synod of Dort. Still, the controversies of the period within the church remained for the most part local and diffuse. For an annotated bibliography, see Peter Milward, *Religious Controversies of the Jacobean Age* (London, 1978).

²⁶ Downname, *Hosea*, 2d pagination, 261, 328, 329. The law-suit motif is prominent in Kimchi. See Joannes Mercerus, *Commentarii locupletiss. in Prophetas quinque priores inter eos qui minores vocantur* (Geneva, 1583). Kimchi's *Duodecim prophetæ cum commentariis* was also available in a Paris edition of 1539–40. The motif also appears in expositions of Hosea by Occolampadius, Calvin, Capito, Tremellius, Gualther, Pareus, Farnow, Lambert, and others that may have been known to Downname, though not in Drusius's *In prophetam Hoseam lectiones* (Leyden, 1601), which he cited. Typically, the Geneva Bible note to Hosea 4:1 has Hosea summoning the people before God's judgment seat.

²⁷ Downname, *Hosea*, 2d pagination, 106.

seems to have occurred to theologians whose deity not only was “far mightier” than his people but necessarily “had the law and equity on his side.”²⁸

By the time Downname’s text enabled him to bring the people into court it had already given him the opportunity to work out his country’s corporate relation with God as nation and church. He made no ado about it. Pacing methodically in the prophet’s steps, he came upon the matter at Hosea 1:10–11, where the “children” of Israel and Judah (according to Paul) are to be gathered in the millennial day, thought it through, and then passed on, leaving little to suggest that he had even recognized a problem. Still, the moment was crucial. Not only did Downname’s discourse, from this point forward, require and rest on the formal definition of Israel, but his handling of the subject also opens to our view the very heart of the paradigm—the nature and destiny of England as God’s nation.

Israel, Downname found, had five distinct meanings in Scripture. (1) Sometimes the word denoted the whole posterity of Jacob “according to the flesh.” (2) At other times it referred to the ten tribes, excluding Judah and Benjamin but taking in every individual member of the northern ten, “both elect and reprobate, believers and unbelievers.” (3) The term could also designate only those Jews who descended from Abraham, the common father of them all, by “natural generation” but not by “spiritual regeneration”—God’s people in the flesh but not in faith. Here Downname, building on Paul’s doctrine of Abraham’s two seeds, was thinking of the Ishmaelites, the graceless posterity of Abraham by his bondwoman Hagar. (4) Still following Paul, Downname found that *Israel* was sometimes limited to the “true Israelites” of both the flesh and the spirit (far fewer and infinitely better than the Ishmaelites), who sprang from Abraham by his wife Sarah and shared his faith in Christ the Messiah. These, too, were parties to the controversy, but they would be pardoned whereas the Ishmaelites would be executed. And (5) there were “Israelites” who were not Jews at all but belonged to God solely by the spirit. This figurative Israel contained all faithful gentiles down to the present day, whom the Messiah would accept as “sons of the living God” (Hosea 1:10) and would gather into his true church together with the faithful Jews. England’s Christians came into the church and the paradigm through this last door. Downname rejoiced at that, because, by virtue of the calling of the gentiles, “the mercies of God have as free a course unto us as ever they had to the Jews”—in fact, a good deal freer, since Christians had a far clearer sight of Christ and were not burdened with the ceremonial laws of Judaism. But not all England, any more than all Jewry, could squeeze into the sanctuary. Downname made clear that when Hosea spoke of and to the “children of Israel,” God’s true sons, he had in view not the people at large but the elite class of the elect.²⁹

All this so far was good enough for the godly; but it left open the question of how Israel could be paradigmatic for the English people as a national whole. As gentiles, the English were not Israelites “according to the flesh,” but neither could they be—all of them, collectively—Israelites by courtesy of the spirit. In fact, none of the five classes fitted their case. Since the first four contained only Jews, only the fifth could

²⁸ *Ibid.*, 49, 50, 61, 55.

²⁹ *Ibid.*, 1st pagination, 96–97, 105.

cover England; but it could do so only if the whole population were regenerate. Put to that question, the Hoseas—close students of the national soul—replied that most of the people were but nominally Christian; in Perkins's words, "a great part of them are Ishmaelites."³⁰ If England had supplanted Israel in God's heart, the reason was certainly not that the inhabitants were so advanced in grace and goodness as to constitute an "elect nation," going to heaven in a crowd.

Stating the problem another way, we observe that Downname's exegesis brings into view—and into tension—the key components, on man's side, of God's relations with humanity. At the head of Hosea 1:10 are the "children," the true church. Below them are the "people," for whom God has a special but not a saving regard. Going with the flow of Reformed theology, Downname ran a line of grace from Abraham, the prototypical Christian, to Abraham's English children-in-the-spirit. He had then to show how the rest of the English people—the greater and worse part of them—had any share in God's interest and therefore in the benefits and burdens of the paradigm. Whether one spoke of England or Israel, only a few of the people were "children"; how, then, could either nation en bloc "be said to be . . . the people of God?"³¹

Downname's answer married the paradigm to the covenant divinity of his day.³² The significance of this conjunction is apparent when it is contrasted with Protestant thought prior to Downname's exegesis on the subject of God's covenants with men. Three generations of theologians before Downname had employed the concept of the covenant of grace to unite the testaments, certify the continuity of Providence, and confirm the unbroken life of the church from the postlapsarian promise to Adam and Eve (Genesis 3:15) through the ages of Abraham, Moses, and David, past Calvary, and onward in space and time to embrace the godly of Protestant Europe and England. Before the Incarnation this covenant conveyed the promise of Christ to come, made the messianic message known by types, ceremonies, and prophecies, and used the moral law to convict and convert sinners. After the Resurrection the moral law continued to serve as rod for sinners and rule for saints, although at no time, either before or after Christ, did the works it required have salvific value. It could thus be said that the two dispensations or testaments of grace were one in substance—justification by faith alone—but advanced over time (in the terminology of Jeremiah and Hebrews) from the "old covenant" of law and types to the "new covenant" that came with Christ and would continue to the last day. This program for the *Heilsgeschichte* combined the ends of grace with the means of the law; in certain theological circles it still has life today as a form of the effort to explain God's ways.

By the close of the sixteenth century, Reformed theologians had begun to find it hard to hold within this plan the two constituents whose central principles seemed not merely different but antipathetic—the law, whose conditionality balanced salvation on the knife edge of a quid pro quo, and the gospel, whose efficacy

³⁰ Perkins, *A Commentary or Exposition upon the First Five Chapters of the Epistle to the Galatians* (1604), in his *Works*, 2 (Cambridge, 1617): 300.

³¹ Downname, *Hosea*, 1st pagination, 106.

³² This paragraph and the two following are a synopsis of my "Grace and Works," 463–502.

depended on the perfect purity of undeserved and unconditioned grace. Dichotomistic to a fault, late sixteenth-century Calvinists not only raised high the standard of double predestination but began to break apart the system of covenant. Sensing in the conditionality of the law a threat to the gratuity of grace, yet unwilling to abandon the covenant design, resourceful minds devised for the law a separate category, which they called the covenant of nature or works. The doctrinal and spiritual purpose was to insulate grace against contamination by legalism. The polemical aim was to prove against papists the futility—more, the fatality—of trying to work one's way to heaven. The evangelical desire was to take the curse off the law in order to make the law fully effective in the operations of redemption. Accordingly, these revisionists packed conditionality, legalism, and works-righteousness all together into the basket of their new idea.

The covenant of works received its power from the covenant of the law, which it embodied and enforced. Conceived (theologically speaking) at Sinai, it soon found a prior site in Eden, on the premise that Adam and Eve had the moral law of nature written on their hearts. There in the Garden, on the first day of man's creation, was played out the drama of the making and breaking of the covenant. God offered life and death on terms; the parents of humanity in effect chose death. The covenant of works thus joined forces with the decree of reprobation. English divines—puritans especially—took a bold hand in brushing up this dark side of Calvinism. Downname caught the covenant of works at a fairly early stage in its evolution, while it was still largely a synonym for the Mosaic law, though already beginning to assume the Adamic flesh-tones of depravity, death, and damnation.

The elaboration of the covenant coincided with the ascent of the paradigm. English thinkers took the lead along both of these converging lines of practical divinity, arguably because the same impulses that prompted them to promote the paradigm also opened their eyes to the utility of framing the nation's relations to God in covenantal terms. The Old Testament taught them that a people were not chosen against their wills by force or fear, but came freely to God. The covenant supplied the nexus of the action. God offered his covenant, as in Exodus 19, and, when the people collectively accepted it, they committed themselves wholly to its conditions: God would accordingly preserve them as long as they obeyed and worshipped him. The covenant of the law could thus be conceived as a national contract founded on popular consent, the constitutional principle and basis of peoplehood. But to say it this way is to project the implications of the thought beyond the times and beyond the minds inhabiting them. Whatever the covenant's historic role as an incubator of republicanism, Jacobean preachers valued the idea for quite different reasons—as an instrument of divine rather than popular sovereignty and as a rationale for ruliness that fused public order with moral reform. Reinforcing the paradigm, the covenant offered to increase the ministers' leverage on a people who had willingly submitted themselves to the rule of law and whose choice of a god brought them by their own act under the authority of that god's earthly agents.

Although a few preachers had hinted at an engagement of the paradigm with the

covenant,³³ no one until Downname gave dogmatic blessing to the match. It could never be doubted that God was English, but the connection had not been plainly stated as a connection of covenant since the only kind of covenant available, until recently, had been the covenant of grace. Unquestionably, the Church of England possessed a kind of grace that came from being Protestant, but even the most ardent apologist had never claimed that all church members would be saved; and, if the church were spotted, what could be said of the inhabitants of the land generally, the obdurate parties to God's controversy? There was, in short, an impediment, even an impasse: the paradigm could not thread the needle of covenant divinity, the eye being narrowed by Calvinist exclusivity.

Downname broke through all this. In view of the advance of covenantal thinking in his time, it is not surprising that he brought the English people into that form of relationship with God, but he did it with a difference. He might, like other exegetes, have introduced the covenant of the law with a mind to lay the commandments on England's Israel. And so, indeed, he did. But he went farther, plain puritan that he was, and with a puritanical disgust for equivocation told the people that the covenant that made them God's and dictated the terms of God's controversy with them was none other than the covenant of works.

It was done quietly, almost casually. If we had not known there was a question—in what precise scriptural sense were the people of England God's people?—we would not recognize an answer in Downname's gloss on Hosea 1:10–11. A people could be said to be God's, he explained, in three ways: by the decree of election, by being received into the covenant of grace, or by admission to the covenant of works. He then stripped away the alternatives. The Israelites as a nation had not been tied to God by election: there could be no such thing as an elect nation—universally and collectively saved—and, in any case, God had finally rejected the nation of the Jews. Neither could they be his by right of the covenant of grace, for that covenant, like the decree, extended only to the favored few. It remained irreducibly to conclude that the Jews had been God's people through the covenant of works. By this stiffly conditional agreement they had been weighed and found wanting. They bore to God a relation that law-bound theologians of Downname's time, treating the Bible as a casebook, defined in rigidly contractual terms that magnified God's justice, diminished his loving-kindness, and reflected a very emaciated sense of holiness and awe.

This exegesis gave covenantal consequence to Paul's distinction of the seeds, meshed the paradigm of Israel with the covenant of works, and provided a theological matrix for the empirical data of God's complaint against England. When Downname finished with Hosea 1, he had defined the "children" and the "people" and shown how to place each group within the frame of covenant divinity.

³³ See Michael McGiffert, "Covenant, Crown, and Commons in Elizabethan Puritanism," *Journal of British Studies*, 20 (1980): 49. I would like to correct my error there that has Perkins conflating the national covenant with the covenant of grace. Notions of stretching the covenant of grace to cover the nation—aired, for example, by the puritan George Widley in *The Doctrine of the Sabbath* (London, 1604), 190–91—were fanciful and ineffectual.

Both were equally parties to the paradigm for terrestrial purposes: no one at that time appears to have suggested, as some did later, that the godly might reasonably expect to have more of the good and less of the bad in this life than their unsanctified neighbors. At the same time, while collected into the paradigm as one nation and one church, the two sets were parted for spiritual purposes by a wall that became as high and thick as the difference between the covenants of works and grace. In this respect, their estates were reckoned exceedingly unequal: on the one side, the greater and worse part; on the other, the smaller and better. Downname discovered a “secret opposition” between “the covenant of works, made between [God] and all the Israelites, and the covenant of grace, made between him and all the faithful.” The one was temporary, the other perpetual.³⁴ The difference rose from the inevitable violation of the law by those who lived by it and would therefore die by it, as Israel had done.

The event is ironic: we are observing the subversion of the paradigm by the very Scriptures on which it was founded. The paradigm had formerly served the ends of national unity; it continued to do so, yet—to the degree that preachers followed Downname’s lead and divided their Israel with the double-bladed texts of the covenant—the concept became transformed from the mirror of the nation into the shibboleth of a party. In retrospect, when Downname, speaking for the “children,” consigned the “people” to the covenant of works, he pointed the way toward what, in the perspective of this analysis, may properly be called the Puritan Revolution.

Nothing could have been further from his mind. He can best be understood as intending to lay an exegetical basis for the national charter of morality, using the covenant to ensure the paradigm. In splitting Israel, he did not explicitly write off the people, nor did he close God’s controversy by making the covenant of works pronounce their sentence of death. On the contrary, he maintained the principle of the inclusivity of the national church, and it seems likely, though the argument is impressionistic, that men of his time and temper found support in the covenant of works for their loyalty to that principle. The mutual pledges of the covenant had constituted Israel a national church, compassing the whole people and mingling Abraham’s sound seed with a great heap of chaff and husks. The visible English church was comparably muddled—stained in principle and practice with the impurity so revolting to clear-eyed separatists. It was thus an accurate copy of the Old Testament model. In a word, England’s church succeeded Israel’s because it embodied the constitutional imperfections of comprehensiveness. Men such as Downname would conform, for all their reservations, and would resist separation, not only because their church passed the New Testament apostolic tests of grace but also because it rested on the Old Testament paradigm of Israel, backed by the covenant of works. Churchmen did not voice this thought, yet it lay embryonically enfolded within the discovery that God had a controversy with his people in their ecclesiastical as well as civil state. It was left to the antinomians in the future to let

³⁴ Downname, *Hosea*, 1st pagination, 294–96. Also see Downname, *Christian Warfare* (2d edn., London, 1609), 681. There Downname, working from Hosea 2:19–20 (God’s unbreakable covenant with the elect), drew the same differences between the covenants of works and grace and explicitly assigned Israel to the covenant of works.

the secret out; and their charge that the church not only taught but also lived upon the covenant of works gained a dangerous credibility from the rise of the Arminian party within the church.

By bringing England under the covenant of works, Downname gave that concept a chance to gain a grip on the moral ideology of nationalism; but we have seen enough of the idea to know that it could not do so. To be sure, as long as it remained identified with the fundamental moral law, it could buttress the inculcation of national morality by adding man's consent to God's command. The preachers found it problematic, however, as it hardened into a rationale for death and damnation. As law, the covenant showed the people where they had gone wrong and supplied incentives to do right; in this role it had a positive part in the controversy. But when changed from indictment to sentence—a sentence both final and capital—it brought the controversy to a fatal close and thus became morally dysfunctional. William Perkins had observed that the legal covenant propounded justice without mercy, and in the waning years of James's reign a follower of Perkins, William Pemble, spelled out the point in lectures at Magdalen Hall, the Oxford headquarters of puritanism. Defending *sola fide*, Pemble juxtaposed the covenant of works as “a compact of pure justice” initiated with Adam against the covenant of grace as “a compact of freest and purest mercy.” Under these heads he contrasted law and gospel to show that the law held out no hope of salvation. This, he told the students, was “most manifest” in God's “renewing of the first covenant of works with the Jews” at Sinai. God would keep them if they kept it; but Pemble had read God's mind and could report that he “did not intend that the Jews should obtain salvation by obedience to that law,” for he “knew well enough that they were never able to keep their promise.”³⁵ The covenant of works in this form left no hope to any man or nation.

This sample of the puritan's higher casuistry may have salved the English Christian conscience vis-à-vis the Jews with the balm of holier-than-thou, but it can hardly have led the preachers to encourage their countrymen to grasp the covenant of works in expectation of reward. It is therefore not surprising that Richard Rogers's large (103 sermons) and well-regarded commentary on Judges (published in 1615) sharply distinguished the conditional national covenant from the absolute covenant of grace but never called it a covenant of works, or that the elder John Brinsley in his *The Third Part of the True Watch*—a tract of 1622 that gives the most complete account of the paradigm in the whole history of the genre—declared that Israel's “covenant is ours as well it was theirs” but did not prejudice it with the label of works.³⁶ As a platform for national morality (or for morality of any description), the covenant of works was a gallows with a trap. The contemplation of its fatality—hence its strategic debility—quite possibly explains why such men as Rogers and Brinsley did not press the Israelite paradigm in that direction.³⁷ Like Downname,

³⁵ Perkins, *Galatians*, 299; and Pemble, *Vindiciae Fidei: or, A Treatise of Justification by Faith* (London, 1625), 23, 140.

³⁶ Rogers, *A Commentary upon the Whole Book of Judges* (London, 1615), esp. 91–92; and Brinsley, *The Third Part of the True Watch* (1622; 2d edn., London, 1623), 135.

³⁷ The Adamic connection had not yet become normative; as it did, the fatality of the covenant became oppressive. For the process in a nutshell at an early date, see Henry Finch, “Of the Covenant of Works,” in his

and Perkins too, they chose rather to keep in the foreground the positive suasions of the law. The preachers who took cues from Downname and built the Hosead followed the same line. Virtually without exception, when they taught the law, they kept the covenant of works out of sight, although their sense of the future of the major part of the English nation was darkened by its sentence of death.

THE EXPOSITION OF HOSEA 4:1–3 flowed from the exegesis of Hosea 1:10–11 as use from doctrine. Speaking to the “children,” God canvasses the sins of the “inhabitants” or “people,” among whom for this purpose the “children” are understood to be comprised. Downname used the passage to create a compendium of wickedness. Sins against God (the four commandments of the first table) came under the want of “knowledge of God”; sins against man (the six of the second) clustered under “no truth nor mercy” in verse one and branched into the particulars of verse two. Downname explained that Hosea followed the Bible’s “usual method” of making the “external works” of the second table the touchstone for the “spiritual and inward graces” of the first, to the scandal of the pharisees. At the same time, he maintained that breaches of the first table—impieties arising from willful ignorance of God’s laws—were “the root of all sin” and the ultimate “cause of all punishment.”³⁸ It was therefore the preacher-prophet’s primary task to instill the “knowledge of God.”

The lesson proceeds under the tutelage of the law as a comparative exercise. “Let us examine what similitude there is between the land of Israel, as it was in the time of Hosea, and our own country in these days, and whether we are not guilty of the same sins, . . . to the end that if we prove clear and innocent we may have the comfort hereof in our own consciences, or if we be found alike faulty that we may humble ourselves by unfeigned repentance.”³⁹ Downname’s either/or was only a gesture, for of course the issue was moot and the conclusion foregone. God would not try England were England’s guilt not already sealed. It was only a matter of putting the evidence on record, and the generous format of the commentary enabled Downname to take the measure of a text that was itself remarkable for amplitude. Thus the Hosead diagrammed England’s evil, showed the figure of

The Sum of Sacred Divinity, ed. Downname (London [before 1621]), which Downname edited for the press. Finch began the chapter by enduing the covenant of works with the beauty of holiness of men and angels at their creation. They ended it, the Fall intervening, by showing that this same covenant enacted “the whole curse of the law, . . . which, because in our corrupt estate it is impossible for men to [keep], therefore this covenant is said to engender children unto bondage (Gal. 4:24)” as slaves of sin and Satan; *ibid.*, 223, 226.

³⁸ Downname, *Hosea*, 2d pagination, 63–65. Here is an early inkling of the esteem for the first table that J. Sears McGee has seen as a hallmark of puritanism; McGee, *The Godly Man in Stuart England: Anglicans, Puritans, and the Two Tables, 1620–1670* (New Haven, 1976). Nothing in the Hosead, or anywhere in the preaching of the paradigm, supports Sacvan Bercovitch’s undocumented assertion that “in Europe . . . the jeremiad pertained exclusively to mundane, social matters, to the city of man rather than the city of God”; Bercovitch, *The American Jeremiad* (Madison, Wisc., 1978), 9. Bercovitch’s rhetorical flourish gives no help to an argument for American exceptionalism that cannot be made to stand on these grounds. On the contrary, the continuity of the transatlantic form is impressive. Bercovitch’s account of the American jeremiad fortunately does not depend on the mistake of holding that its “fusion of secular and sacred history” differentiated it in principle from its English antecedents; *ibid.*

³⁹ Downname, *Hosea*, 2d pagination, 68.

God's justice in the pattern, and laid out an agenda for preachers of the paradigm.⁴⁰

Downname put a very broad construction on Hosea's particulars. *Swearing* connoted all sins against God, and *killing* signified "all hurts, wrongs, and injuries" to neighbors; all sins of omission were comprised in "the want of knowledge, as being the fountain of all the rest," while every sin of commission came under the head of "abusing God's holy name," because "he that maketh no conscience of blaspheming . . . maketh conscience of no sin."⁴¹ To catch all sinners in the net, Downname showed that the least transgression—he instanced the social lie of the servant who tells an unwelcome caller that the master is out, together with the lie of the master who authorizes the deception—deserved nothing less than hellfire.⁴² Sinning, moreover, was not the foible of the few but the habitual pastime of all the people. Accordingly, Downname raked the realm without distinction of rank, office, or occupation: court, city, country; magistrates, lawyers, shopkeepers, yeoman—all were sunk. He was strong against the squeezing of the poor—"where amongst any civil people was ever seen the like tyrannical oppression whereby the rich and mighty eat up the weak and needy as the greater fishes devour the less?"—but saved his keenest stabs for clergymen who betrayed their office, the frothy wits, dumb dogs, and benefice-engrossers who fleeced but did not feed the flock.⁴³ Sin, like gangrene, crept through "the whole body of the commonwealth" from the headmen of state, church, and society to all the lesser and lower members.⁴⁴ The argument from Hosea 4:1–3 frames this multitude of evils into a bill suitable to the image of one nation under God, indivisible in guilt. When Downname was done, he had all England crowded onto one scaffold and all necks in one noose.

What if the nation did not repent and reform? Downname's reading of Hosea 4:3 held out no hope of clemency: if England failed to mend its ways, "the Lord will bring this land to utter destruction."⁴⁵ What form would vengeance take and when might one expect it? Hosea had foretold the extinction of the Northern Kingdom by Assyrian arms: the "whole commonwealth . . . and church of the Jews" would be destroyed; most of the people would be slaughtered and the remnant led away into bondage; the conqueror would plant colonies in the land to keep the refugees from ever coming home again.⁴⁶ So, too, for England—but Downname did not linger on doomsday. Hosea, he noted, had many "dark speeches" and obscure lines "fit for a prophecy, which is as a book sealed up until it be opened and expounded by the event."⁴⁷ It was not for any man to break into God's secrets. Beyond that, the entire record of the paradigm shows that its preachers practiced the craft of suspense. They did not need the wisdom of serpents to recognize that catastrophe, sighted

⁴⁰ Downname himself pursued several of the charges in *Four Treatises, Tending to Dissuade All Christians from Four No Less Heinous than Common Sins—Namely, the Abuses of Swearing, Drunkenness, Whoredom, and Bribery* (London, 1609), as well as in *The Plea of the Poor* (London, 1616) and *A Treatise against Lying* (London, 1636).

⁴¹ Downname, *Hosea*, 2d pagination, 78, 81, 90.

⁴² *Ibid.*, 92, 99–100.

⁴³ *Ibid.*, 71, 83, 103.

⁴⁴ *Ibid.*, 93. But Downname's judgments became acrophobic as his criticisms ascended the ladders of power.

⁴⁵ Downname, *Hosea*, 2d pagination, 107.

⁴⁶ *Ibid.*, 126–27.

⁴⁷ *Ibid.*, 337.

too soon, would blunt the paradigm's edge. Downname said as much when he remarked that the worst inflictions were not sudden and swift—bad as these were—but grindingly slow, and not so much material as spiritual. At the last, when God gave up on a people, he simply left them to simmer in their sins.⁴⁸

THROUGH 1625, IN ADDITION TO DOWNNAME'S TREATISE, Hosea furnished texts for a number of sermons or sermon series that contributed to God's controversy as *amici curiae*. No other prophet rivaled him, and few if any Scriptures can have been more frequently preached than Hosea 4:1–3. Arranged by date, the sermons of the Hosead include

George Webbe, *God's Controversy with England* (1609), preached at Paul's Cross (Hosea 4:1–3);

William Ward, *The Sinner's Indictment* (1612; Hosea 4:1–3);

Thomas Sutton, *England's Summons* (early 1613), preached at Paul's Cross (Hosea 4:1–3);

Samuel Smith, *An Exposition upon the Sixth Chapter of the Prophecy of Hosea* (1616), a series preached in Essex (Hosea 6);

John Fosbroke, *England's Warning by Israel and Judah* (1617), preached at Paul's Cross (Hosea 5:15);

William Loe, *Vox Clamantis—Mark 1:3: A Still Voice to the Three Thrice-Honorable Estates of Parliament and in Them to All the Souls of the Nation*, published in 1621 but preached to Parliament some years earlier (Hosea 5:1–2);

Daniel Featley, *Pandora's Box: or, The Cause of All Evils and Misery*, printed in 1636 preached to the High Commission about 1620 (Hosea 13:9); and

Henry Leslie, *A Warning for Israel* (1625; Hosea 14:1–2, handled as an epitome of the whole prophecy).

From the early Caroline period there is, in addition, Samuel Torshell, *God's Controversy for Sin*, collected in his *The True Saint's Humiliation* (1633). Among the undeterminable number of unprinted sermons is one by a Mr. Wood on Hosea 4:2, preached at the Spital in Easter week, 1624, and mentioned a few days later by a Paul's Cross preacher as giving notice that "the Lord may take up his own complaint against this city."⁴⁹

⁴⁸ *Ibid.*, 166.

⁴⁹ John Lawrence, *A Golden Trumpet to Rouse Up a Drowsy Magistrate . . .* (London, 1624), 78. Also possibly pertinent are two later sermons on Hosea 4:4: Henry Bennet's *A Pastor's Plea for Peace* (1640), listed by Crowe but apparently not extant, and an attack on *jus divinum* episcopacy by one Coxo of the Exeter diocese that drew Laud's ire; see Crowe, *Catalogue*, 121; and William Laud, *Works*, ed. G. W. Scott and James Bliss, 6 (Oxford, 1857): 369. God's controversy popped up often and warm in the pulpit of the period. See, for example, Alexander Udny, *The Voice of the Cryer* (London, 1628), 14: "As Hosea said to Judah, so I say to Britain, *Hear the word of the Lord, ye children of Britain*, for the Lord hath a controversy," etc. Hosea also supplied text and theme for the preaching of the paradigm, on lines other than God's controversy, by George Benson, William Jackson, and Isaac Bargrave. Henry Finch found in Hosea a prophecy of the millennial calling of the

The Hoseas ranged across the church from the Laudean bishop Leslie to the puritan Smith, a member of the Earl of Warwick's clerical stable who lived long enough to be ejected at the Restoration. Paul's Cross—"the chiefest watchtower in the land," Webbe called it⁵⁰—provided a central platform, but the livings of most of the ministers lay afield in Oxfordshire (Webbe), Leicestershire (Ward), Essex (Smith), Northamptonshire (Fosbroke), Cheshire (Torshell), or Dublin (Leslie). Their diversity supports the finding that the Hosead did not articulate nonconformity but was rather the undifferentiated expression of churchmen who wished most earnestly to speak with one voice. The historian seeking a touchstone of dissent will not find it here: Smith alone excepted, this preaching buried contention over ceremonies and polity in commitment to a common code of morality and in consensus on homiletic formula, figure, and strategy.

Insofar as the Hoseas needed a foundation, they built to an appreciable extent on Downname's commentary. The book was cited by other ministers—at a time when such references were rare save in polemical tracts—and their silent borrowings from it also show their esteem. Webbe hoped that his Paul's Cross sermon would not be "prejudiced" by Downname's work or, he added, by the fact that "these same words have been often taught, peradventure unto your ears."⁵¹ He drew freely on Downname for thought and phrase. Sutton lifted liberally from both Downname and Webbe. (After Sutton's untimely drowning ten years later, Downname married his young widow.) Featley, the independent rector of Lambeth Chapel who (with Loe, another strong anti-Laudean) led the fight against Arminian innovations at Charles's first Convocation and nearly two decades later broke from the Westminster Assembly on the issue of episcopacy, borrowed Downname's five-fold definition of Israel in order to explain how God could simultaneously threaten and bless his people.⁵² Torshell pushed the judicial metaphor to a point of Latinate technicality (God "begins with a *venire facias*. If we answer not, he sends a *capias pluries*") where he felt obliged to caution himself not to "trifle or play." At Bunbury, Cheshire, in Downname's father's former diocese, Torshell held his living from the Haberdashers on Downname's nomination. Quite typically of the Hoseas, he inclined toward puritanism but did not turn against episcopacy until the early 1640s.⁵³

Jews; Finch, *The World's Great Restoration* . . . (London, 1621), esp. 179–234. A quite different use is illustrated by Samuel Garey's *Britannia Vota*, which glorifies James as Joash, dilates on the submission of subjects, and (following the text, Hosea 7:5) cautions the king to keep his head among his councillors. To Garey, it was cause for satisfaction but also some anxiety that "God hath set England as it were upon an hill, a spectacle to all nations," with James the king of the hill. Garey, *Britannia Vota: or, God Save the King*, in his *Great Britain's Little Calendar* (London, 1618), 82.

⁵⁰ George Webbe, *God's Controversy with England: or, A Description of the Fearful and Lamentable Estate Which This Land at This Present Is In* (London, 1609), A3v.

⁵¹ *Ibid.*, 11.

⁵² Daniel Featley, *Pandora's Box: or, The Cause of All Evils and Misery*, in his *Clavis Mystica* (London, 1636), 86. For Featley's and Loe's opposition to Laud, see William Leo [sic], *A Sermon Preached at Lambeth, April 21, 1645, at the Funeral of That Learned and Polemical Divine, Daniel Featley* . . . (London, 1645).

⁵³ Samuel Torshell, *The Saint's Humiliation* (London, 1633), 56. See DNB, s.v. "Torshell, Samuel." Robert Horn cited Downname's *Hosea* as a model in his *The Christian Governor in the Commonwealth and Private Families* . . . (London, 1614), A7r. Abraham Gibson drew on it in his *The Land's Mourning for Vain Swearing* . . . (London, 1613). Its influence is hinted in T. R.'s "Epistle to the Reader" in B. P., *The Prentice's Practice in Godliness*, A6r, and in B. P.'s text, *ibid.*, 17r. (This work was reissued in 1619 with the initials W. P. in place of B. P. on the title page, and a new title: *The Young Man's Guide to Godliness: or, The Prentice's Practice on Earth That Hopes for a Freedom in*

The Jacobean Hosead resembled the general run of paradigm preaching of the period in being politically innocuous. The king was still David (never mention Jonathan); his courtiers, though faulted for foppery in dress and other decadence, were not found to have Machiavellian hearts; the prelates were bad, some of them, but prelacy was not. None of the Hoseas was prepared to give up either his calling or his country. England had not yet come to the crisis point where Thomas Hooker, seeing God “packing up his gospel” and beginning to “ship away his Noahs” to New England, showed from Hosea 1 how “the Lord is said to dis-church or discharge a people.”⁵⁴ Most Hoseas did not pursue this line. Although they argued with zeal, they did nothing (Smith excepted) to widen Downname’s division between “people” and “children” or to develop the schismatic potential of his covenantal classification. Moderate and meliorist, they remained wedded to the hope that, if magistrates and fellow ministers would do their duty, if the great ones and better sort would take the van, if the godly would rally behind them, if words or blows could get the people up and moving, God would suspend judgment yet awhile. In a word, in its Hoseatic mode the paradigm worked as it was meant to—not to put an end to England (either mundane or millennial) but to give incentives to live and strive.⁵⁵

WELL BEFORE CHARLES CAME TO THE THRONE OF LAUD to the high altar, changes in the presentation of the paradigm foretold the agitations of the new regime. The rhetoric grew curt; the rage and hurt cut deeper. The preachers began to exhibit a baffled incredulity: How could men, how could Englishmen, be so incorrigibly bad, and how could God in justice, taking such abuse, suffer so long? How could this nation have had so much preaching with so little to show for it? In fact, there may have been a good deal more to show than the ministers could admit (although we are not called to referee God’s controversy or to appraise their sociology of sin), but they were trapped in the terrible logic that made each gain—one Sabbath more, one stage-play less—the basis for a higher demand, because God’s successive mercies and reprieves raised the nation’s moral obligation and vitiated its excuses. Under this grinding, with the people apparently falling and the preachers certainly rising, the paradigm grew sharp. Quite without knowledge or intent, reformers were whetting a blade for the necks of the troublers of the children of Israel. Before the 1620s were out, the chopper was at the door. “We acknowledge,” said grim Alexander Leighton in 1628, “that God hath a special controversy with his people (which if it were taken up, he would quickly dispatch his enemies). But our point [is] to prove our sins and judgments to have their rise from the hierarchy, and them to be the capital sin.”⁵⁶

Heaven.) The nonconformist William Hinde, who preceded Torshell at Bunbury, put Hosea 8:12 at the head of his anti-antinomian treatise on *The Office and Use of the Moral Law of God in the Days of the Gospel* (London, 1623). On Torshell and Hinde, see Seaver, *The Puritan Lectureships*, 148, 161.

⁵⁴ Hooker, *The Danger of Desertion*, in George H. Williams et al., eds., *Thomas Hooker: Writings in England and Holland, 1626–1633* (Cambridge, Mass., 1975), 246, 230.

⁵⁵ Although, as Downname noted, one of Hosea’s purposes was to foretell the Messiah, God’s controversialists did not dwell on this theme and took only very marginal parts in the millenarian swellings of their period. They were concerned with immediate problems for which solutions were still to be worked out in historical time.

⁵⁶ [Leighton] *An Appeal to the Parliament: or, Zion’s Plea against the Prelacy* ([Holland?] 1628), 154.

We may catch a glimpse of this future as early as 1616 in the preaching of Smith, the most puritanical of the Hoseas.⁵⁷ Like his colleagues, Smith judged the whole people guilty, but the subtitle he gave to his sermons on Hosea 6, setting off “the true repentance of the godly” from “the hypocritical repentance of the wicked,” points to different intentions and sense of situation. He was much less concerned to apply the paradigm corporately to the nation than to teach the godly how to fend off or sidestep God’s displeasure and keep on a right parallel with him. With a growing number of his kind, Smith adapted the model of Israel to the specifically tribal interests of Downname’s fifth class. Two decades earlier, Richard Hooker had rebuked such presumptuous persons for arrogating to themselves alone the biblical precedents of divine favor.⁵⁸ Smith’s *Sixth Chapter of the Prophecy of Hosea* helped signal the revival and fresh advance of this differentiating and invidious spirit. Then and increasingly thereafter, when one finds a puritan in the paradigm, this is his sign: from the church’s mint he takes the common coin of grace and stamps on it his own image of godliness.

It might seem to the credit of the preachers that the wicked were repenting, even if only in hypocritical show; but for men like Smith sham sanctity created a pastoral problem: they found it harder and at the same time ever more urgent to separate the “children” from the “people.” Their situation was ironic: the more converts gained, the more cause to worry lest the conversions be false. To the degree that Smith and his kind were correct in perceiving growing numbers of men and women flocking to their standard, they could persuade themselves that their preaching of the paradigm was having the right effect. But, if recruits regularly turned out to be frauds, then the preachers had to think again, for in that case the program was not working as it should. Then they had to sharpen their search for symptoms of hypocrisy (the harder they looked, the more they found), while their penchant for purity deepened into obsession. Thus was set in train a reordering of priorities that focused clerical attention more and more narrowly upon the godly and gave an ideological spring to the puritan party of the church of Laud.

The paradigm was at this point fitted in the name of godliness to an agenda of militant piety that began by hinging the saving of the kingdom on the welfare of the saints and rose as Laud gained ground toward the conclusion that the saints must save themselves, let England fare as it might. Making ever more strenuous use of covenantal imperatives and sanctions, the prosecutors of God’s controversy split the paradigm along the cutting line between the covenants of works and grace. Soon enough, extremists like Thomas Hooker were speaking of Noah and Lot, of ark and exodus, of preserving the few from the doom of the many. The controversy, they believed, had not long to run. The people of England would not be pardoned; God’s English children must either escape or seek shelter and trust to be passed over when God came to strike.

⁵⁷ Smith, *An Exposition upon the Sixth Chapter of the Prophecy of Hosea, Wherein Is Set Down the True Repentance of the Godly, as Also the Hypocritical Repentance of the Wicked, Most Needful in These Times* (London, 1616). This work is not listed in the *Short Title Catalogue* or the *Dictionary of National Biography* for the only Samuel Smith who appears in those guides, but our Smith is very probably that same Smith—and the same Smith, too, who has a place in Calamy’s roster of ejected ministers.

⁵⁸ Hooker, *The Laws of Ecclesiastical Polity* (London [1597]), 41.

Smith in 1616 was already preparing to sink the people in the covenant of works. The “cause of all calamity,” he declared, was the crime of breaking covenant with God, and all men by nature were covenant breakers.⁵⁹ But it made a difference of life and death which covenant one broke. Those who broke the covenant of works without compunction were lost beyond recall: England, take warning! Those who broke the covenant of grace, though prodigals, were still within God’s care. They could not break his covenant without being in the covenant, and, being in it, they could not get out of it short of committing ultimate apostasy. From this disparity of status, as deep as the difference between the curse of the law and the grace of the gospel, arose a calculus of righteousness in which membership in the covenant of grace became the puritans’ absolute measure of men for the great threshing of England. This had been potential in the model of Israel from the outset, but strain was needed to give it political effect. At last, the “children” found themselves settling God’s controversy out of court, upon the battlefield. Such was, *mutatis mutandis*, the unexpected and unwanted upshot of the injunction that John Downname had embedded in the Hosead: “Therefore let us not think it enough that God hath outwardly made his covenant with us, unless it be also written in our hearts. . . . For unless we live like his people and servants, he will not acknowledge us for such, but will cast us off as he did the Israelites.”⁶⁰

⁵⁹ Smith, *Sixth Chapter of the Prophecy of Hosea*, 260 (irregular), 270.

⁶⁰ Downname, *Hosea*, 1st pagination, 90.

Natives and Immigrants, Free Men and Slaves: Urban Workingmen in the Antebellum American South

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OF NECESSITY, HISTORICAL RESEARCH IS PIECEMEAL. But the growth of historical understanding is not simply an additive process. Instead, new discoveries transform older conceptions and necessitate reconsiderations at the highest interpretive level. A re-evaluation of the place of immigrant workers in antebellum Southern cities suggests how the process works, for it forces a reformulation of ideas about the entire structure of the urban work force and raises questions about the nature of Southern society, urban and rural, and the beliefs of Southerners, native and immigrant, free and slave.

FEW OF THE BULWARKS OF SOUTHERN DISTINCTIVENESS have withstood the battering ram of historiographic iconoclasm better than the overwhelmingly native origins of the Southern people. While scholars of all persuasions have challenged the unique character of the South's economy, its social structure, its politics, and its institutions, the weight of historical evidence has left this mark of Southern difference undisturbed.¹ Even a passing acquaintance with transatlantic migratory patterns

An early version of this paper was prepared in 1975–76 while its authors were fellows at the Shelby Cullom Davis Center for Historical Study at Princeton University. They remain indebted to the center's director, Lawrence Stone, for encouragement and support. Rebecca Scott ably assisted with the initial research and analysis. Judith Rowe and Nita Roberts of the Princeton University Computer Center and Bertha Butler of the Computer Science Center at the University of Maryland guided the statistical aspects of this study. Another version of this paper was thoughtfully criticized at the 1981 Annual Meeting of the Organization of American Historians by Harold Woodman. Research grants from the Ford Foundation and the Rockefeller Foundation facilitated additional research, and we thank Robert Schrank and Joel Colton, respectively, for making this work possible. We also want to acknowledge the thoughtful criticisms offered by W. Elliot Brownlee, George Callcott, Eric Foner, Ronald Hoffman, Michael P. Johnson, Bruce C. Levine, Sidney Mintz, Joseph P. Reidy, and Leslie S. Rowland.

¹ For a recent attempt to deny the distinctive nature of Southern society, see Edward Pessen, "How Different from Each Other Were the Antebellum North and South," *AHR*, 85 (1980): 1119–49; also see the comments that follow in "AHR Forum—Antebellum North and South in Comparative Perspective: A Discussion," by Thomas Alexander, Stanley L. Engerman, Forrest McDonald and Grady McWhiney, and Edward Pessen, *ibid.*, 1150–66.

affirms that few of the thousands of Europeans who journeyed to the United States in the years before the Civil War settled in the slave states. At mid-century, when fully one Northerner in seven had been born outside the United States, only 5 percent of the Southern free population was foreign born, and a disproportionate number of these resided in the border states. Ten years later, the margin had widened as European migrants surged into the North, while they continued to dribble into the South. On the eve of the Civil War, when fewer than one free Southerner in fifteen had been born outside the United States, immigrants composed nearly a fifth of the population of the free states.²

Historians have found this massive imbalance reason enough to ignore immigrants in antebellum Southern society and to argue that slavery repelled foreign settlers. While lavishing attention on the role of foreign-born people in the transformation of pre-Civil War Northern society, scholars have neglected their place in the development of the antebellum South. This is particularly true in accounts of the immigrants' part in the making of the urban working class. Although the portrait of antebellum class formation in the cities of the North has been constructed as a rich ethnic mosaic, in the urban South the process has been etched in black and white.

The conventional treatment of Southern urban workers leaves little room for immigrants—indeed, for white labor generally. Drawing on the antislavery argument, particularly that of political abolitionists, historians have emphasized that the presence of slave labor degraded free white workers. With William Seward, they have maintained that slavery denied the white workingman employment and expelled him “from the community because it cannot enslave and convert him into merchandise.” The rules of the free market economy simply did not apply to labor in a slave society. As Charles Nordhoff, another opponent of slavery, observed, it mattered nothing to the slaveholder “how low others can produce the article; he can produce it lower still, so long as it is the best use he can make of his [slaves'] labor, and as long as that labor is worth keeping. A free white mechanic is at the mercy of his neighbor, the capitalist, in a slave state, because, if the capitalist does not like the price, he can go and buy a carpenter and sell him again when the work is done.” Conceding that slaves worked at lower rates than free laborers, historians have concurred in the abolitionist argument that, “when the two are brought into competition, white labor is crowded out.”³

² U.S. Bureau of the Census, *The Seventh Census of the United States, 1850* (Washington, 1853), xxxvi–xxxviii, and *Population of the United States in 1860* (Washington, 1864), 620–23. If slaves are included in the analysis, the proportion of immigrants in the Southern population shrinks (to about 3 percent in 1850, 5 percent in 1860), and the differences between the proportion of immigrants in the North and that in the South grows. For a survey of recent scholarship about immigrants in the Old South, see Randall M. Miller, “Immigrants in the Old South,” *Immigration History Newsletter*, 10 (1978): 8–14. Also see the discussions in Clement Eaton, *The Growth of Southern Civilization* (New York, 1971), 150–76, 221–70; Ella Lonn, *Foreigners in the Confederacy* (Chapel Hill, N.C., 1940), 1–32; Herbert J. Weaver, “Foreigners in the Ante-Bellum Towns in the Lower South,” *Journal of Southern History*, 13 (1947): 62–73, and “Foreigners in Antebellum Mississippi,” *Journal of Mississippi History*, 16 (1954): 151–63; William L. Barney, *The Secessionist Impulse: Alabama and Mississippi in 1860* (Princeton, 1974), 26–43; and Edward L. Ayers, *Vengeance and Justice: Crime and Punishment in the Nineteenth Century* (New York, 1984), chap. 3.

³ Nordhoff, *America for Free Working Men* (New York, 1865), 1–39; and David Bertelson, *The Lazy South* (New York, 1967), 201. For an important exception to this conventional wisdom, see Carville Earle and Ronald Hoffman, “The Foundation of the Modern Economy: Agriculture and the Costs of Labor in the United States and England, 1800–60,” *AHR*, 85 (1980): 1055–94.

TABLE 1
Selected Southern Urban Populations, 1850–1860

City	Total Population in 1860	Percentage of Increase/Decrease from 1850 to 1860	Percentage of Population in 1860		
			White	Free Black	Slave
RICHMOND	38,000	+38	62	7	31
CHARLESTON	41,000	– 6	58	8	34
MOBILE	30,000	+43	71	3	26
NASHVILLE	17,000	+67	77	4	19
LYNCHBURG	7,000	–18	56	5	39
BATON ROUGE	5,000	+27	68	9	23

NOTE: Percentages here and in all subsequent tables do not always add to 100, owing to rounding; total population rounded to thousands.

SOURCES: U.S. Bureau of the Census, *The Seventh Census of the United States, 1850* (Washington, 1853), and *Population of the United States in 1860* (Washington, 1864).

According to the extant historiography, black freemen and bondsmen did most of the labor in the South, including most of the artisanal work. In his classic study, Charles H. Wesley put the proportion at about 80 percent. Others have suggested the same without Wesley's precision. Historians have maintained that native-born white workingmen fled the slave states when they could. Those who remained in the South protested slave competition but had little success against the potent combination of slaves, who monopolized skilled labor, and masters, who reaped a handsome profit from their slaves' work. Such circumstances could hardly encourage foreign immigration, and those few feckless migrants who alighted in the slave states generally found employment at tasks that masters deemed too dangerous for their valuable slaves.⁴ Foreign-born workers have thus appeared marginal, rather than central, to an understanding of the working population of the urban South.

The dominance of the urban work force by black laborers has been challenged implicitly by recent work on urban bondage, which emphasizes the sharp decline of slavery in the cities in the late antebellum years. But scholars have posed the question of urban slavery far too narrowly. They have vigorously debated whether the peculiarities of urban bondage pushed or the nature of plantation demand pulled slaves out of the cities, while ignoring the character of the urban work force and the relations between slaves and free workers. Even the most detailed scholarship has left the immigrant the South's invisible man.⁵

Yet the peculiar pattern of European migration and settlement in the slave states gave immigrants importance far beyond their numbers and projected foreign-born workers into a place in the Southern working class that rivaled the role played by

⁴ Wesley, *Negro Labor in the United States* (New York, 1927), chaps. 1–3; Herman Schlüter, *Lincoln, Labor, and Slavery* (New York, 1913); Kenneth M. Stampp, "The Fate of the Southern Antislavery Movement," *Journal of Negro History*, 28 (1943): 10–22; Roger Shugg, *Origins of Class Struggle in Louisiana* (University, La., 1939), 20–110 but esp. 86–92; Robert S. Starobin, *Industrial Slavery in the Old South* (New York, 1970), 146–89 but esp. 153–63; and Frederick Law Olmsted, *Journey in the Seaboard Slave States* (New York, 1856), esp. 193.

⁵ Richard C. Wade, *Slavery in the Cities: The South, 1820–1860* (New York, 1964); and Claudia D. Goldin, *Urban Slavery in the American South, 1820–1860: A Quantitative History* (Chicago, 1976).

TABLE 2
Occupational Distribution of Employed Free Adult Men, 1860

City	Number	Percentage of Employed Adult Free Men		
		Non- workers	Skilled Workers	Unskilled Workers
RICHMOND	7,954	38	42	20
CHARLESTON	6,985	45	35	20
MOBILE	7,306	38	30	32
NASHVILLE	4,307	41	35	24
LYNCHBURG	1,273	48	34	18
BATON ROUGE	887	40	47	13

SOURCE: Computed from the U.S. manuscript census schedules, 1860, Records of the Bureau of the Census (Record Group 29), National Archives, Washington [hereafter, U.S. Census MSS., RG 29].

immigrants in the North. Although a comparatively small number of Europeans migrated to the South during the nineteenth century, those who did generally settled in cities. South Carolina, Georgia, and Alabama had few immigrants, but Charleston, Savannah, and Mobile had many. At mid-century, the foreign-born population of these states reached 3 percent only in the case of South Carolina. But more than a fifth of Charleston's residents, more than a quarter of Savannah's, and almost a third of Mobile's had been born outside the United States.⁶ Moreover, since men formed a disproportionately large share of the foreign settlers in Southern cities, immigrants composed a still larger share of the urban male work force than they did of the urban population as a whole. As a result, like Mobile (where half of the employed free men had been born outside the United States), many urban places in the Lower South could be appropriately described as immigrant cities.⁷

The same processes that drew immigrant men disproportionately to Southern cities concentrated them in specific places in the social structure of the urban South. As a group, immigrants entered Southern society at the bottom of the free social hierarchy and made up a large part of the lower ranks of urban society. On the eve of the Civil War, native-born white men composed better than two-thirds of the male merchants, political officials, and professionals in Mobile, Charleston, and other Southern cities, while foreign-born men equaled a similar proportion of petty proprietors—grocers, restaurateurs, stable keepers, and the like. Immigrants were

⁶ J. D. B. DeBow, *Statistical View of the United States* (Washington, 1854), 399. This disjunction between rural and urban patterns of immigrant settlement also existed in those areas of the South that received the largest numbers of foreign settlers. In 1860, immigrants composed 13 percent of Louisiana's population but foreign-born men and women accounted for almost two-fifths of the inhabitants of New Orleans; U.S. Bureau of the Census, *Population of the United States in 1860*, 615.

⁷ The data on nativity of employed free men in Mobile, along with similar information of employed men in Baton Rouge, Charleston, Lynchburg, Nashville, and Richmond have been calculated from a tabulation of all men for whom either an occupation or property (real or personal) is listed in the 1850 and 1860 manuscript censuses, the 1850 and 1860 manuscript slave schedules, and the 1850 and 1860 manuscript industrial schedules. National Archives, Washington, Record Group 29.

TABLE 3
Skill Distribution of Free Working Men, 1860

<i>City</i>	<i>Number of Free Working Men</i>	<i>Percentage Skilled</i>	<i>Percentage Unskilled</i>
RICHMOND	4,929	68	32
CHARLESTON	3,846	63	37
MOBILE	4,552	49	51
NASHVILLE	2,533	60	40
LYNCHBURG	661	66	34
BATON ROUGE	529	79	21

SOURCE: Computed from the U.S. Census MSS., RG 29.

also disproportionately represented among urban workingmen, the largest occupational group in every major Southern city. Immigrants commonly dominated the free male working population and formed a large proportion of the entire male working population, free and slave. For that reason, foreign settlers helped shape social relations in the urban South and had a profound influence on class and racial relations throughout Southern society.

The composition of the urban male work force on the eve of the Civil War suggests the significance of immigrant workers in Southern cities.⁸ Four Southern cities and several towns have been studied: Richmond, the South's premier industrial city with a population of some 38,000 in 1860, of which more than 60 percent were whites, about 30 percent were slaves, and less than 10 percent—the remainder—were free Negroes; Charleston, an older Atlantic port with a population slightly larger but of similar composition to that of Richmond; Mobile, an expanding cotton port of some 21,000 whites, 7,500 slaves, and fewer than 1,000 free people of color; and Nashville, a rapidly expanding interior marketing and transporting center, three-fourths of whose residents were white. In addition, two smaller cities have been examined: Lynchburg, a regional Virginia tobacco manufacturing center of 7,000, with proportionally few immigrants, and Baton Rouge, a Louisiana river town of 5,000, with proportionally many immigrants. Except for Charleston and Lynchburg, these cities grew rapidly during the antebellum period. In some respects, their populations increased more rapidly than those of Northern cities.⁹ (See Table 1) In these selected cities and throughout the urban South, laboring men were sharply divided by legal status, nativity, and race. Native-born white men monopolized occupations in the upper ranks of Southern society. The vast majority of merchants, bankers, factors, doctors, and lawyers in these cities

⁸ This essay focuses on the role of men in the Southern urban working population. We are preparing a companion essay that examines the composition of the female sector of the work force of Southern cities. The presence of large numbers of slave women, almost all of whom worked, had a profound impact on the role of free working women and throws into sharp contrast the lives of all women in Northern and Southern cities. In this essay, unless otherwise stated, work force and working population refer to *male* work force and *male* working population.

⁹ Leonard P. Curry, "Urbanization and Urbanism in the Old South: A Comparative View," *Journal of Southern History*, 40 (1974): 43-60.

TABLE 4
Employed and Propertied Adult Free Men by Nativity and Race, 1860

City	Number	Percentage by Nativity and Race				
		<i>Southern- Born White</i>	<i>Northern- Born White</i>	<i>Foreign- Born White</i>	<i>Free Black</i>	<i>Unknown Origin</i>
RICHMOND	8,122	51	7	31	8	3
CHARLESTON	7,256	41	6	42	8	3
MOBILE	7,457	31	16	50	2	1
NASHVILLE	4,415	50	13	31	3	3
LYNCHBURG	1,295	70	3	16	7	4
BATON ROUGE	927	44	11	39	5	1

SOURCE: Computed by the U.S. Census MSS., RG 29.

were white men born in the slave states, frequently in the states in which they resided. Generally, the more successful—as measured by wealth and slaveownership—the greater the likelihood that these men had been born in the South. At the bottom of free society, the free laboring population was a good deal more heterogeneous. Native-born Southern whites, Northern-born whites, foreign-born whites of various nationalities, and free Negroes (almost all of whom had been born in the South but who divided among themselves by their degree of racial admixture) formed the free working class. The remaining portion of the laboring population, the slaves, was also almost entirely native born and, like the free Negroes, divided by color—black and brown. By its legal status, nativity, and race, each group played carefully defined and usually distinctive roles within the urban work force.¹⁰ Free workers constituted a majority of the adult free men in all cities studies. Except in Mobile, skilled workers predominated among these free wage earners. (See Tables 2, 3, and 4.)

On the eve of the Civil War, immigrants composed a large portion of the free urban work force—sometimes stretching to a clear majority. Generally, the further south the city, the greater the proportion of workers of foreign birth. In Mobile, that classic immigrant city, almost two-thirds of the free workingmen were immigrants. Although the proportion of the foreign-born free workers was smaller in Charleston and Baton Rouge, it still equaled about half of the free work force. Immigrants were not nearly as prominent in the cities of the upper South, especially in land-locked interior cities like Lynchburg. Even in Richmond and Nashville, however, foreign settlers totaled 40 percent of the free workingmen. (See Table 5).

¹⁰ For purposes of this essay, workingmen have been divided into two categories: skilled and unskilled. Among those classified as unskilled are day laborers, boathands, draymen, dock workers, sailors, and tobacco factory workers. The skilled workingmen have been further divided into two subcategories: those working in the building trades (including bricklayers, carpenters, painters, plasterers, plumbers, and stonemasons), and other skilled workers (including bakers, barbers, blacksmiths, bookmakers, brewers, butchers, carriage makers, cabinet makers, clock makers, confectioners, engravers, founders, jewelers, pilots, printers, shoemakers, tailors, tanners, wheelwrights, and watchmakers).

TABLE 5
Adult Free Workingmen by Nativity and Race, 1860

City	Number	Percentage by Nativity and Race			
		Southern- Born White	Northern- Born White	Foreign- Born White	Free Black
RICHMOND	4,929	39	8	39	14
CHARLESTON	3,846	28	6	52	14
MOBILE	4,552	19	14	64	3
NASHVILLE	2,533	42	12	41	5
LYNCHBURG	661	62	4	20	14
BATON ROUGE	529	35	11	47	7

SOURCE: Computed from the U.S. Census MSS., RG 29.

Black freemen and Northern-born whites also played an important part in the laboring population of the urban South. Free Negroes or free people of color—the specific nomenclature depended on the region—made up a small and variable portion of the urban working class in every Southern city. In some places, like Charleston and Richmond, they constituted an important minority of the free workingmen—amounting to more than one in seven. Elsewhere, however, they slipped to numerical insignificance. In Mobile, free men of color totaled only 3 percent of the free workingmen. Northern-born white workers also varied widely in number from place to place, although they played a more important role in the newer cities of the west and the Lower South than in the older ones in the Upper South. They, too, equaled about 4 to 14 percent of the free work force. Taken together, free Negroes and Northern-born whites totaled about a fifth of all free workingmen in Southern cities. (See Table 5.)

Although the proportion varied from city to city, immigrants, free Negroes, and Northern-born whites together constituted nearly three-quarters of the free workingmen in Charleston and about 60 percent of those in Richmond and Nashville. In Mobile, immigrants, Northern-born whites, and free people of color totaled a full 80 percent of the that city's free workingmen. Except in the interior marketing and manufacturing center of Lynchburg, white workers native to the South made up only a minority of the urban South's free working class. Within the context of an overwhelmingly native-born regional population and the predominately native origins of the upper ranks of urban society, native-born Southern white workers were a conspicuous minority in most Southern cities. (See Table 5.)

Whatever the dynamics of slavery in the city, enslaved black laborers remained an indispensable part of the urban work force on the eve of the Civil War. Although their share of the total urban labor force shrank during the late antebellum years, nearly all slaves did manual labor of one kind or another, and they remained a large component of the laboring population everywhere except in the border cities. In Charleston, Richmond, Mobile, and Nashville, adult slaves still

TABLE 6
Workingmen by Status, Nativity, and Race, 1860

City	Number	Slave	Percentage of Workingmen			
			Southern- Born White	Northern- Born White	Foreign- Born White	Free Black
RICHMOND	9,557	48	20	4	20	7
CHARLESTON	7,887	51	14	3	25	7
MOBILE	7,002	35	13	9	41	2
NASHVILLE	3,408	26	31	9	30	4
LYNCHBURG	1,623	60	24	2	8	6
BATON ROUGE	843	37	22	7	29	5

NOTE: For the classification of workingmen, see note 10. above.

SOURCE: Computed from the U.S. Census MSS., RG 29.

constituted between one-half and one-quarter of the workingmen, skilled and unskilled.¹¹ In Lynchburg, they constituted over 60 percent of the workingmen. The presence of slaves reduced proportionately the weight of various groups of free workingmen in the urban working class and made Southern whites (like everyone else) a still smaller proportion of the whole. Only a minority of Southern urban workers had been born in the South and shared the most prized Southern attributes: whiteness and freedom. Most urban workers were born alien to the dominant characteristics of Southern culture. (See Table 6.)

Slaves, free Negroes, immigrants of various nationalities, Northern-born whites, and Southern-born whites played different roles in the Southern urban work force. By the various combinations of color, status, and nativity, workers toiled at different skill levels, practiced different trades, and labored in different sectors of the economy. (See Table 7.) For free workingmen, these differences can be discerned by disaggregating the census enumerations. The antebellum census, however, provides no occupational designations for slaves—an omission that has fueled erroneous speculation about the kind of work slaves did in Southern cities.¹² So many free men (immigrants and native born) practiced skilled crafts in Southern cities that it is unlikely urban slaves could have been similarly skilled. Had slaves possessed skills in the same proportion as free workers, the ranks of unskilled labor and domestic service would have gone unfilled and Southern cities would have been like no others in the Western world. An analysis of patterns of slave ownership and employment on the assumption that a slave's work was related to his master's—that an adult slave man owned by a blacksmith was likely to be a blacksmith, while an adult slave man owned by a banker was not—and within the context of the known occupational structure of the free working class confirms this inference. It also provides a rough estimate of the level of urban slave skill and a still rougher guide to the character of urban slave occupations.

¹¹ All slave men between age fifteen and sixty are presumed to have worked and are included in the work force. For a more extended discussion of slave occupations, see pages 1185–87, below.

¹² A recent example is Robert William Fogel and Stanley L. Engerman, *Time on the Cross: The Economics of American Negro Slavery*, 2 vols. (Boston, 1974).

TABLE 7
Free Workingmen, Skilled and Unskilled, by Nativity, and Race, 1860

City	Number	Percentage of Workingmen			
		Southern- Born White	Northern- Born White	Foreign- Born White	Free Black
A: Skilled Free Workingmen					
RICHMOND	3,341	48	10	36	6
CHARLESTON	2,413	38	6	40	16
MOBILE	2,211	25	17	54	3
NASHVILLE	1,522	46	17	33	4
LYNCHBURG	434	70	5	18	7
BATON ROUGE	416	36	12	44	8
B: Unskilled Free Workingmen					
RICHMOND	1,588	20	4	46	30
CHARLESTON	1,433	11	5	72	11
MOBILE	2,341	14	12	69	4
NASHVILLE	1,011	35	4	51	9
LYNCHBURG	227	46	2	25	27
BATON ROUGE	113	34	5	57	4

NOTE: For the classification of workingmen, see note 10, above.

SOURCE: Computed from the U.S. Census MSS., RG 29.

An examination of slave ownership in Mobile, Charleston, and Nashville reveals that most artisans (here including those who might classify themselves as manufacturers employing skilled workers—founders as well as blacksmiths, contractors as well as carpenters) did not own slaves, and those who did controlled only a small portion of the city's slave population. Workers were the largest occupational group, but slave ownership by workers (almost always artisans) never exceeded 15 percent of the slave men. (See Table 8.) Moreover, women (usually adolescent girls) composed a disproportionate share of artisan-owned slaves, suggesting that, when a skilled worker purchased a slave, the slave was usually a servant for the owner's household rather than a journeyman for his shop.¹³

With slave prices rising rapidly in the 1850s, slave hiring grew more commonplace throughout the urban South. Many free workers may have been among those who rented rather than purchased slaves. Both masters and slaves found slave hiring profitable. It provided slaveowners—particularly widowed women—a steady

¹³ For example, in 1860 only 40 percent of the slaves employed by Richmond's white men (including tobacco manufacturers) were women, but women accounted for 60 percent of the slaves employed by white artisans. A similar pattern can be found in Lynchburg, another city where the distribution of slaves was reported by employer rather than by owner. Elsewhere, the disparity between the white male ownership and the white artisan ownership was smaller, and, in Charleston, artisans proportionately owned more men than the white male population as a whole. See note 16, below.

TABLE 8
 Ownership or Employment of Adult Slave Men, 1860

City	Total Number of Slaves	Merchants, Planters, Professionals, and Politicians	Occupation of Owners or Employers						Free Blacks	Unknown
			Manu- facturers	Petty Entre- preneurs	Artisans	Unskilled Laborers	White Women			
RICHMOND ^a	4,628	22	58	6	6	1	5	0	3	
CHARLESTON ^b	4,042	50	0	4	14	1	22	1	8	
MOBILE ^b	2,450	60	5	4	8	1	17	0	5	
NASHVILLE ^c	877	38	5	23	11	3	10	0	9	
LYNCHBURG ^a	962	31 ^d	53	6	4	0	2	0	4	

^a Slaves listed by their employers.

^b Slaves listed by their owners.

^c Slaves listed in some wards by their employers, in some wards by their owners.

^d Includes eighty-four slave men, about 8 percent of the male slaves, employed on the railroad.
 Source: Computed from U.S. Census MSS., RG 29.

income, and it gave slaves an added measure of control over their own lives. Slave hiring increased despite a variety of complaints from white workers fearful for their jobs and white residents fearful for their lives.¹⁴ But slave hiring did not enlarge artisan employment of slaves in most cities. In Richmond and Lynchburg (where census takers noted slave employment rather than slave ownership), large tobacco manufacturers hired most of the slaves. When viewed from the perspective of employment rather than ownership, artisan control of slaves increased slightly. But throughout the urban South, merchants, professionals, and manufacturers (in factory towns like Richmond and Lynchburg) remained the largest employers of slave labor, and, for the most part, these employers required unskilled, rather than skilled, laborers.¹⁵ (See Table 8.) While some merchants and manufacturers may well have employed slave artisans, some free artisans doubtless worked their slaves in menial roles. In any case, when slave artisans—estimated as all slave men owned by free white artisans—are placed within the context of the artisan class, their share of the skilled work force exceeds 8 percent in Charleston but nowhere else. Even when the estimated number of slave artisans is doubled (that is, every slave man owned by a white artisan plus an equal number owned by others), slaves still composed less than 15 percent of the artisan population in Mobile, Richmond, and Nashville. Thus, as a general rule, urban slaves appear to have toiled in either the most backward sector of the economy as domestic servants and day laborers or in the most advanced sector of the economy as factory hands. (See Table 9.)

Only in Charleston did slaves make up a large portion of the mechanic class. Charleston artisans owned slaves in numbers that proportionately nearly doubled the rate of artisan slave ownership in Mobile, Richmond, or Nashville. Moreover, unlike the pattern of artisan slave ownership in those cities, Charleston artisans held slave men in disproportionate numbers.¹⁶ Those Charleston artisans who did not own slaves also had ample opportunity to hire them, since city-bound planters and white women owned a larger proportion of their city's slave population than in any of the other places investigated. Thus a tradition of slave artisanry reaching back into the eighteenth century—built upon the high level of skill demanded by the rice economy and sustained by a cultural milieu more akin to the West Indies than the mainland—existed in Charleston on the eve of the Civil War.¹⁷ If every slave man owned by a Charleston artisan practiced his master's trade, Charleston's slaves

¹⁴ For discussions of slave hiring, see Wade, *Slavery in the Cities*, 38–54; Starobin, *Industrial Slavery in the Old South*, 128–37, 211–12; Clement Eaton, "Slave-Hiring in the Upper South: A Step toward Freedom," *Mississippi Valley Historical Review*, 46 (1959–60): 663–78; and Richard B. Morris, "The Measure of Bondage in the Slave States," *ibid.*, 41 (1954–55): 219–40.

¹⁵ For the best discussions of Southern tobacco manufacturing prior to the Civil War, see Joseph C. Robert, *The Tobacco Kingdom: Plantation, Market, and Factory in Virginia and North Carolina, 1800–1860* (Durham, N.C., 1938); and John O'Brien, "Black Richmond, 1850–1870" (Ph.D. dissertation, University of Rochester, 1978), chap. 1. Also see Starobin, *Industrial Slavery in the Old South*.

¹⁶ In 1860, 51 percent of the slaves owned by Charleston's white men were women, but only 39 percent of the slaves owned by white artisans were women. See note 13, above.

¹⁷ Artisan ownership of slaves was commonplace in post-Revolutionary Charleston, the largest slave city in the nation. In 1790, more than half of Charleston's artisans owned slaves, and their slaves amounted to nearly a quarter of the city's slave population. The nature of the available evidence makes it impossible to determine how many of the artisans' slaves were men, but the size of their holdings suggests that many used slaves in their shops as well as in their homes. For example, 60 percent of Charleston carpenters owned slaves. Almost three-quarters of these slaves were held in units of six or more, units large enough to extend beyond the domestic

TABLE 9
Skill Distribution among Free and Slave Workingmen, 1860

	Skilled	Unskilled	Skilled	Unskilled	Skilled	Unskilled	Skilled	Unskilled	Skilled	Unskilled	Skilled	Unskilled
	<i>Richmond^a</i>		<i>Charleston^b</i>		<i>Mobile^c</i>		<i>Nashville^d</i>		<i>Lynchburg^e</i>		<i>Baton Rouge^f</i>	
NUMBER	3,619	5,938	2,978	4,909	2,407	4,595	1,612	1,797	473	1,150	451	392
PERCENTAGE												
Southern-born White	45	6	31	3	23	7	43	20	63	9	33	10
Northern-born White	9	1	5	2	16	5	16	2	4	0	11	2
Foreign-born	33	12	32	21	50	36	31	28	16	5	41	16
Free Black	5	8	13	3	3	2	3	5	6	6	7	1
Slave	8	73	19	71	8	49	6	44	10	80	8	71

	Skilled	Unskilled	Skilled	Unskilled	Skilled	Unskilled	Skilled	Unskilled	Skilled	Unskilled	Skilled	Unskilled
	<i>Richmond^g</i>		<i>Charleston^h</i>		<i>Mobileⁱ</i>		<i>Nashville^j</i>		<i>Lynchburg^k</i>		<i>Baton Rouge^l</i>	
NUMBER	3,896	5,661	3,544	4,343	2,603	4,399	1,710	1,698	521	1,102	485	358
PERCENTAGE												
Southern-born White	41	6	26	4	21	8	41	21	58	9	31	11
Northern-born White	8	1	4	2	15	6	15	3	4	0	10	2
Foreign-born	31	13	27	24	46	37	29	30	14	5	38	18
Free Black	5	9	11	4	2	2	3	5	5	6	7	1
Slave	14	71	32	67	15	47	11	40	18	79	14	68

ASSUMPTION: The percentage of slaves who were skilled—that is, if x percent of slaves were skilled, then the skill distribution among workers by status, nativity, and race was as described in the corresponding table— a = 6 percent; b = 14 percent; c = 8 percent; d = 11 percent; e = 5 percent; and f = 11 percent; g = 12 percent; h = 28 percent; i = 16 percent; j = 22 percent; k = 10 percent; and l = 22 percent.

SOURCE: Computed and estimated from the Census MSS., RG 29.

would have contributed nearly a fifth of the skilled workingmen in that city. If in addition to those owned by artisans, other slave men also engaged in skilled labor, the proportion of artisanal work performed by slaves would have been higher still. In Mobile, Richmond, and Nashville, all of which developed during the nineteenth century, far fewer slaves practiced skilled trades, and skilled bondsmen composed only a small proportion of the mechanic class. Although urban slaves everywhere enjoyed greater mobility and cultural autonomy than their counterparts in the countryside, only in Charleston—and cities where similar conditions existed—did large numbers of bondsmen practice skilled trades. Elsewhere, few seem to have escaped the dull, demeaning, and backbreaking work that also characterized slave labor in the countryside.

Although slaves were the single largest source of unskilled labor in Southern cities, many free men also worked at unskilled menial jobs. Assuming that all slave men not employed as artisans worked as laborers, slaves supplied slightly better than 70 percent of the unskilled and service workers in Richmond and Charleston, 50 percent in Mobile, and more than 40 percent in Nashville. In short, even where slaves performed most of the unskilled work, free workers still composed a substantial minority of the unskilled laborers. In some slave cities, like Nashville, free workers did most of the unskilled, menial work performed by men. (See Tables 7B and 9.)

Irish immigrants generally dominated the ranks of unskilled urban free men throughout the urban South. In Charleston, fully 60 percent of the free unskilled laboringmen had been born in Ireland, and, although that proportion slipped substantially in Mobile, Richmond, and Nashville, it remained above 40 percent. In occupational terms, the nativist slander that an Irishman was a “nigger” turned inside out contained a considerable element of truth in such places. Immigrants of other nationalities, along with free Negroes, accounted for most of the remainder of the unskilled free workingmen. (See Tables 7B, 10.)

But, even more than among workingmen generally, native-born whites were conspicuous in their absence from the ranks of the unskilled; and, in the cities of the Lower South in particular, it would have been difficult to find a Southern-born white man shouldering a shovel or lifting a hod. Unskilled Southern-born whites constituted no more than 8 percent of the employed free men in the Southern cities studied. In Charleston, fewer than one Southern white man in fifty—compared to more than half of the Irishmen—did unskilled labor. (See Table 11.) While the structure of the urban laboring class confirmed the degraded status of blacks and identified the Irish with the slave, native-born Southern whites insulated themselves from such imputations.

needs of most households and to contain several slave men. Some Charleston carpenters held so many slaves that their number almost compelled commercial exploitation. A full 25 percent of slaveowning carpenters owned ten or more slaves. These estimates result from linking the 1790 Charleston manuscript federal census and the 1790 Charleston city directory. The directory lists occupations, and the census gives the number of slaves residing in individual households. Any computation of slave ownership (using the federal census before 1850), however, is compromised by the fact that some slaves within a given household may not have been owned by the head of the household; such computations must, therefore, be considered rough estimates. Jacob Milligan, comp., *The Charleston Directory* (Charleston, 1790); U.S. Bureau of the Census, *Heads of Families at the Time of the First Census of the United States, Taken in the Year 1790* (Washington, 1908), 31–34.

TABLE 10
Irish and Free Negro Men in the Free Work Force, 1860

City	<i>All Free Workingmen (Percentage)</i>		<i>Unskilled Free Work- ingmen (Percentage)</i>	
	<i>Irish</i>	<i>Free Negro</i>	<i>Irish</i>	<i>Free Negro</i>
RICHMOND	18	14	46	30
CHARLESTON	29	14	60	11
MOBILE	28	3	43	4
NASHVILLE	23	5	45	9
LYNCHBURG	—	14	23	27
BATON ROUGE	—	7	31	4

SOURCE: Computed from the U.S. Census MSS., RG 29.

As among the unskilled workers, color, status, and nativity sharply divided skilled workers. Except for the Irish, the vast majority of immigrant workingmen practiced artisanal trades, and immigrant artisans composed a disproportionate share of the skilled urban work force. (See Table 7A.) In Richmond, for example, nine out of ten British and German workingmen practiced skilled trades. Although immigrant workers could be found in almost every urban craft, they were especially important in urban service trades like tailoring and shoemaking. In Mobile and Charleston, four of five free shoemakers had been born outside the United States, a proportion only slightly smaller in Richmond and Nashville. Foreign-born workers similarly dominated blacksmithing in Lower South cities and composed a disproportionate share of the blacksmiths even in the urban Upper South.

Free Negro men contributed to the urban artisan class in a special way.¹⁸ First, they were generally confined to one or two trades that whites had denominated “nigger work” and that were usually identified with servile, dirty, or distasteful labor. Although such trades differed from place to place, barbering and butchering typified such occupations. While free Negroes totaled 3 percent of the skilled free workingmen in Mobile and 16 percent of those in Charleston, they equaled, respectively, 20 and 78 percent of the free barbers in those two cities. Second, mulattoes generally composed a disproportionate share of the skilled freemen. This division between mulattoes and blacks was especially evident in the cities of the Lower South. Men of mixed racial origins composed over three-quarters of Charleston’s free Negro barbers, one of the most lucrative and prestigious free Negro occupations, but less than one-quarter of the free Negro day laborers. Even in Upper South cities, Negro freemen of mixed racial origins enjoyed a higher skill level than those denominated black.

Southern-born white men also held a distinctive place within the ranks of the urban artisan class. They tended to congregate in a few occupations, most notably in the building trades and in crafts like piloting and printing in which native birth provided them with obvious advantages. Generally, over half the Southern-born

¹⁸ Ira Berlin, *Slaves without Masters: The Free Negro in the Antebellum South* (New York, 1974), 217–49; and Leonard P. Curry, *The Free Black in Urban America: The Shadow of the Dream* (Chicago, 1981), 15–36.

TABLE 11
Unskilled Southern-Born White Workingmen, 1860

<i>City</i>	<i>Number</i>	<i>Percentage of All Employed Free Men</i>	<i>Percentage of All Workingmen (Free and Slave)</i>	<i>Percentage of Free Workingmen</i>
RICHMOND	324	4	3	7
CHARLESTON	158	2	2	4
MOBILE	342	5	5	8
NASHVILLE	360	8	11	14
LYNCHBURG	100	8	6	15
BATON ROUGE	38	4	4	7

SOURCE: Computed from the U.S. Census MSS., RG 29.

white artisans labored as printers and pilots and in the building trades. In some places this proportion reached three-quarters. Even in the building trades, however, Southern whites did not always predominate. While more than half the Southern-born white artisans in Charleston and Mobile worked in the building trades, they equaled no more than 40 percent of building-trade workers. In Charleston, about one in three skilled Southern-born white workingmen labored as carpenters, but the typical free Charleston carpenter was either a free Negro, a Northern white, or an immigrant. (See Table 12.) Outside of these trades, Southern-born whites were grossly underrepresented in the artisanal classes of Southern cities.

A work force with a disproportionately large immigrant artisanal sector and an overwhelmingly black and immigrant unskilled sector did not appear suddenly in Southern cities on the eve of the Civil War. Slave labor had characterized Southern urban life from the beginning of settlement, and the number of European immigrant workers had grown steadily during the middle decades of the nineteenth century. Indeed, in the Lower South, the evolution of an urban work force in which immigrants played a central role appears to have taken place well before mid-century. By 1850, immigrants already numbered almost half of the free workingmen of Charleston and three-fifths of those of Mobile—and an even larger proportion of the unskilled free workers in these cities. In both cities, two of three unskilled free men had been born outside the United States. In the Upper South cities of Richmond and Nashville, however, foreign-born workers still played a minority role in the free labor force at mid-century—although a substantial one. During the decade before the war, immigrant workers entered these Upper South cities in large numbers and increased their share of the working class not only at the expense of slaves—as scholars have emphasized—but also at the expense of native-born white workers.

While historians have focused their attention on the proportional decline of urban slavery, the most significant change in the composition of the urban work force was the increase of immigrant and the decline of native white workers. In Nashville, for example, the number of slave men increased by more than 50 percent between 1850 and 1860, but the number of immigrant workingmen

TABLE 12
Free Building-Trade Workingmen by Nativity and Race, 1860

City	Number	Percentage by Nativity and Race			
		Southern- Born	Northern- Born	Foreign- Born	Free
		White	White	White	Black
RICHMOND	933	66	7	20	8
CHARLESTON	876	41	5	32	21
MOBILE	804	30	20	45	5
NASHVILLE	687	64	13	22	1
LYNCHBURG	160	84	2	9	5
BATON ROUGE	188	52	13	23	12

SOURCE: Computed by the U.S. Census MSS., RG 29.

increased threefold, with foreign-born artisans doubling and immigrant laborers increasing by some 500 percent. Meanwhile, the number of native-born white workingmen increased by less than 40 percent, lagging behind the allegedly declining slave population. In some artisanal trades, native-born whites suffered an absolute decline. A similar development took place in Richmond. There the slave population grew by about 20 percent as tobacco manufacturers scoured the countryside for hirelings. But the number of immigrant workers more than doubled, while native white workers increased at a pace akin to those in Nashville. In the Lower South ports of Charleston and Mobile, the composition of the work force changed less dramatically during the 1850s, because the central importance of immigrant workers had already been established. But, in the cities of both the Upper and the Lower South, immigrants made especially large gains in the skilled trades, so that their dominance of artisan work increased even faster than their prominence in unskilled labor. Throughout the urban South, foreign-born workers remained the most dynamic element in the working class and increased their dominant position. (See Table 13.)

Skilled free workers, unskilled slaves, immigrants of all sorts playing roles that historians have previously given over to others—this portrait of the composition of the urban work force brings Southern cities much closer to the description of a Charleston boardinghouse rendered by a British merchant in 1860: “Fully one-half the large number of guests in the House seemed as if they had just stepped out of Houndsditch, and remind me of what a friend in Mobile said, that ‘I should meet more Jews in Charleston than I could see in Jerusalem.’”¹⁹

KNOWLEDGE OF THE COMPOSITION of the wage-earning population of the urban South has profound implications for comprehending the development of Southern

¹⁹ W. Corsan, *Two Months in the Confederate States, Including a Visit in New Orleans under the Domination of General Butler* (London, 1863), 9–10.

society. Just as the new understanding of the rôle of immigrant workers in Southern cities forced a reconsideration of the structure of the entire work force (slave and free), so the new understanding of the South's urban work force necessitates a re-evaluation of the social order of the antebellum South. The reconsideration of urban laboring people raises questions about the development of Southern society, the character of slavery and the evolution of Afro-American culture, and the nature of politics in the urban South and the role of immigrants in

TABLE 13
Increase and Decrease of Urban Workingmen
between 1850 and 1860 by Race, Status, and Nativity

City	Percentage of Increase or Decrease				
	Southern-	Northern-	Foreign-	Free	
	Born	Born	Born	Blacks	Slaves
	Whites	Whites	Whites		
RICHMOND	+37	+97	+166	+42	+27
CHARLESTON	+11	-24	+25	-11	-46
MOBILE	+73	+43	+81	+51	+17
NASHVILLE	+38	+106	+223	+162	+55

SOURCE: Computed by the U.S. Census MSS., RG 29.

the growing sectional controversy. The answers to these questions will ultimately rest on the results of investigations that have hardly begun. Based on this re-evaluation of the composition of the Southern urban working population, some suggestions can be made, if only to indicate the direction that those investigations might take.

First, this analysis of the composition of the urban working population raises questions about the culture of native Southern whites who remained in the countryside, despite opportunity in the city, and of those who migrated to cities but avoided manual work, skilled as well as unskilled. The answers to these questions suggest how the requirements of cotton agriculture and the great prosperity of the late antebellum countryside made yeoman farmers unlikely migrants to the cities. They also suggest the Southern yeomanry's deep attachment to the land and to its own unique culture as well as suggesting the strength of the unspoken entente between planters and yeomen, which guaranteed the security of the yeoman's separate world. It re-emphasizes the importance of understanding the history of rural plain people.²⁰

²⁰ Steven Hahn, *The Roots of Southern Populism: Yeoman Farmers and the Transformation of the Georgia Upcountry, 1850-1890* (New York, 1983); Eugene D. Genovese, "Yeoman Farmers in a Slaveholders' Democracy," *Agricultural History*, 44 (1975): 331-42; and Frank L. Owsley, *Plain Folk of the Old South* (Baton Rouge, La., 1949).

The power of that distinctive way of life often survived the transfer to the city and shaped the lives of country folk in the urban South. As expanding centers of commerce, every Southern city housed a small army of clerks—young, generally unmarried men who boarded together and aspired to counting houses of their own. In overwhelming proportions, these young men had been born in the South, and (given the growth of Southern cities) probably in the countryside. In Charleston, for example, two-thirds of the clerical workers were native to the South and generally to the state of South Carolina, and a similar pattern could be found in other Southern cities.²¹ Thus, differences between those who labored in clerical, commercial occupations and manual, industrial ones were compounded by distinctions of nationality throughout the urban South.

Second, understanding the composition of the work force of the urban South raises questions about the changing character of slavery and its development during the nineteenth century. Slaves had played a far different role in the post-Revolutionary urban South than in the mid-nineteenth century. It is not simply that there were proportionately fewer of them in the cities of the pre-Civil War period, a point that has consumed far too much of recent scholarly debate on the subject. More importantly, during the colonial period slaves had actively participated in almost all major artisanal trades; on the eve of the Civil War, except in a few cities with roots deep in the eighteenth century, they did not.²²

The large number of slave artisans in the post-Revolutionary South helps explain how free Negroes came to enjoy a comparatively high level of skill during the nineteenth century, despite the early presence of politically active white artisans. Most free Negroes obtained their liberty during the period when slaves were still deeply involved in the artisanal economy. They maintained their skills through the antebellum years while slaves were systematically stripped of their crafts. A possible interpretation for the decline of slave skill argues that competition from newly arrived immigrants muscled slaves out of the artisan crafts. Yet, this explanation fails to account for the Negro freemen's ability to maintain their occupational position. In any three-way competition among white, slave, and free black workers, black freemen would probably be the most vulnerable. Yet, free Negroes maintained their occupational position, and the level of free Negro skill appears to have had little connection with the proportion of immigrants in any Southern city. It is possible, then, that the erosion of slave skill began before the great nineteenth-century European migrations and had far more to do with the expansion of the cotton economy than with the entry of foreign-born workers into the South. The opening of new lands to staple production required skilled labor to construct

²¹ Clerical workers include accountants, agents, bookkeepers, clerks, and salesmen. Of the 890 clerical workers in Richmond, 83 percent were born in the slave states; of 1,146 in Charleston, 65 percent; of the 1,278 in Mobile, 71 percent; and, of the 554 in Nashville, 60 percent. This information was computed from the U.S. Census MSS., RG 29.

²² Allan Kulikoff, "Tobacco and Slaves: Population, Economy, and Society in Eighteenth-Century Prince George's County, Maryland" (Ph.D. dissertation, Brandeis University, 1976), 235–39; Gerald W. Mullin, *Flight and Rebellion* (New York, 1972), chaps. 3–4; Philip D. Morgan, "Black Society in the Lowcountry," in Ira Berlin and Ronald Hoffman, eds., *Slavery and Freedom in the Age of the American Revolution* (Charlottesville, Va., 1983); and Louis Morton, *Robert Carter of Nomini Hall: A Virginia Tobacco Planter in the Eighteenth Century* (Williamsburg, Va., 1964). Also see the evidence cited in note 17, above.

quarters for master and slaves and to build and repair barns, carts, presses, and a variety of agricultural machinery. Slave artisans may have been the first to go west, pulled out of the cities in large numbers by the rapid spread of plantation agriculture across the South. In a time of great economic growth, artisan standing provided scant protection from sale. Indeed, it may have encouraged physical removal to distant rural workplaces, a process that reduced the number of slave artisans in cities, transformed urban skills into rural skills, and cut the generational lines by which slaves transferred their crafts. If a Richmond carpenter, who might otherwise pass his skill to his son in Richmond, was sold to Alabama, his son would be left without skill and he would be left without a son to reproduce his skill. The long-term loss of slave skill affected not only urban slaves but rural ones as well. In 1860, according to several estimates, well under 10 percent of rural slave men enjoyed artisanal status.²³

The loss of skilled standing deeply affected the behavior and values of blacks, both slave and free, during the nineteenth century. In a variety of ways, it may have weakened black resistance to white domination. In almost all early working-class movements, the worldliness and confidence engendered by skill propelled artisans to positions of leadership.²⁴ Yet, the destruction of slave skill had other, less obvious—and perhaps even more insidious—effects. It appears to have slowed considerably the rate of self-purchase, a practice common among urban slaves elsewhere in the hemisphere. Without skills, even the most industrious slaves had difficulty buying their way out of bondage. Like the sharp decline of manumission at the beginning of the nineteenth century, the inability of slaves to purchase liberty severed the bonds that had existed between slaves and free blacks at the end of the eighteenth century when slaves moved into the free Negro caste in great numbers.²⁵ Already torn by differences in status, the black community faced still another obstacle to caste unity. Skill differences pushed black freemen and slaves further apart than they would have been if they had shared a common occupational experience.

After emancipation, the distinctive occupational traditions of former free and former slave blacks made those who had enjoyed freedom more important within the larger community yet, paradoxically, hampered their ability to act with authority. Free Negroes provided a disproportionate share of post-Civil War black leadership not only because they had been free but also because they carried the

²³ Roger Ransom and Richard Sutch, "The Impact of the Civil War and Emancipation on Southern Agriculture," *Explorations in Economic History*, 12 (1975): 1–28. In a study based on the mortality schedule of the U.S. Census (1850 and 1860), Michael P. Johnson has calculated the rate of slave skill to be about double the estimate of Ransom and Sutch; Johnson, "Slave Occupations and Marriages," unpublished essay courtesy of the author. For additional evidence on the scarcity of artisanal skills among exslaves during and just after the Civil War, also see Herbert G. Gutman, *The Black Family in Slavery and Freedom, 1750–1925* (New York, 1976), 39–41, 233, 479–83.

²⁴ The classic study of such leadership is E. P. Thompson's *The Making of the English Working Class* (New York, 1963). For the role of American artisans in developing working-class movements, see Alan Dawley, *Class and Community: The Industrial Revolution in Lynn* (Cambridge, Mass., 1976); and Paul G. Faler, *Mechanics and Manufacturers in the Early Industrial Revolution* (Albany, N.Y., 1981). For a cross-cultural comparison, see Bryan Palmer, "Most Uncommon Common Men: Craft and Culture in Historical Perspective," *Labor/Le Travailleur*, 1 (1976): 5–31.

²⁵ Berlin, *Slaves without Masters*, 15–50, 138–60.

artisanal tradition within the black community. Yet former slaves and former freepeople had not known each other well, particularly in the urban South. Fearful of the fate of slave craftsmen, free Negro artisans may well have hesitated to identify fully with the newly emancipated unskilled masses. That most of them took the risk says something about the dynamics of Reconstruction, but in the end their worst fears were realized.²⁶ Slaves did not lose skills as a result of emancipation; they had few to lose. Free Negroes suffered badly. An examination of the occupational structure of the black community in Mobile and Richmond in 1880 indicates that generational lines of craft transfer had been smashed within the black community. Although blacks maintained their pre-Civil War skill level through Reconstruction, the internal composition of the black artisan class changed dramatically. Compared to skilled white workers, black artisans—probably descendants of free Negroes—were top heavy with age in the major crafts. The vestiges of traditional black artisanal skills, which had reached a high point in the late eighteenth century and were maintained through the antebellum period by free Negroes, were liquidated in the last decades of the nineteenth century.²⁷ Artisan-ship and petty enterprise often went together, so that the decline in artisan skills also stifled small businesses. Booker T. Washington thrived in this constricted setting, but urban blacks migrating to Northern cities in the early decades of the twentieth century carried few skills and little business experience with them.

Finally, the composition of the work force of Southern cities raises questions about the immigrants who constituted so large an element in the free working population of Southern cities. Although a full understanding of their place as workers in Southern society requires close attention in particular cities to the ways immigrants lived, where they resided, what institutions they formed, and how they related to others, some preliminary probing suggests the dimensions of the issue.²⁸

In the free states, immigrant workers stood at the center of the most important changes in economy, society, and politics during the late antebellum years. Newly arrived workers, particularly skilled ones, carried artisanal traditions from the Old World to the New, where they fused with the indigenous artisanal traditions of American mechanics and craftsmen. These beliefs, deriving in large measure from

²⁶ Thomas Holt, *Black over White: Negro Political Leadership in South Carolina during Reconstruction* (Urbana, Ill., 1977); David C. Rankin, "The Origins of Black Leadership in New Orleans during Reconstruction," *Journal of Southern History*, 40 (1974): 417–40; Charles Vincent, *Black Legislators in Louisiana during Reconstruction* (Baton Rouge, La., 1976); Loren Schweninger, *James T. Rapier and Reconstruction* (Chicago, 1978); Howard N. Rabinowitz, ed., *Southern Black Leaders of the Reconstruction Era* (Urbana, Ill., 1982); and especially David C. Rankin, "The Impact of the Civil War on the Free Colored Community of New Orleans," *Perspectives in American History*, 11 (1975): 379–416.

²⁷ Gutman, *Black Family in Slavery and Freedom*, 433–60, 476–519, 623–44. By 1880, the percentage of all skilled white workers under the age of thirty in Richmond was 46 percent and all skilled black workers under thirty 30 percent, and in Mobile in the same year the percentages, respectively, were 41 percent and 23 percent; the percentage of all white carpenters under the age of thirty in Richmond was 25 percent and of all black carpenters 9 percent; in Mobile the percentages respectively were 27 percent and 13 percent. This information was computed from the U.S. Census MSS., RG 29.

²⁸ The only book-length study of an antebellum Southern white immigrant and predominantly working-class community is Earl F. Niehaus's *The Irish in New Orleans, 1800–1865* (Baton Rouge, La., 1965). Also see Christopher Silver, "A New Look at Old South Urbanization: The Irish Worker in Charleston, South Carolina, 1840–1860," *South Atlantic Urban Studies*, 3 (1979): 141–72. John F. Nau's examination only begins at mid-century but contains material of interest; Nau, *The German People of New Orleans, 1850–1900* (Liedens, 1958). For a work that concentrates on German political leaders and intellectuals, see Dieter Cunz, *The Maryland Germans: A History* (Princeton, N.J., 1948).

the common character of artisan life and work throughout the Atlantic world, celebrated independence and emphasized a man's right to the fruits of his own labor as central to that independence. Such ideas mixed readily with republican notions about the rights of man and with nationalistic ideas about the rights of American citizens. Together these beliefs drew workers into the ongoing struggle against slavery, as an institution fundamentally opposed to those values that the artisanal experience taught guaranteed freedom. Artisans, some of them nascent manufacturers, played a large role in the rise of political antislavery in the free states, as they not only came to see slavery as a threat to their own liberty but also developed a deep sympathy for the plight of the slave. Such abstract beliefs did not, however, necessarily negate longstanding antipathy toward black people. Indeed, disdain for blacks persisted among Northern white workingmen, reinforced by the historic suspicion of small propertyholders toward the propertyless as well as fear of competition from the emancipated. To accommodate opposition to both slavery and the slave, the abolitionist movement frequently—though not always—advanced with opposition to blacks. Free soilism and Republicanism took both egalitarian and racist forms in the free states.²⁹

Immigrant workers who arrived in the South carried ideas and traditions about the meaning of work and its relation to liberty similar to those carried by workers who migrated to the North. (In fact, many arrived first in the free states and then traveled south, and others moved back and forth between Northern and Southern cities.³⁰) Deeply rooted beliefs about the connection between labor and liberty and cherished ideas about the connections of both to republicanism took on special meaning in a slave society. Although many quickly adopted Southern racial prejudices and some became slaveholders, immigrant workers generally remained suspicious of chattel bondage. Proportionately, immigrant workers owned or employed fewer slaves than did native-born workers. If some workers, skilled and unskilled, ultimately came to see black bondage as protection for white liberty and insurance for their own elevated position in a split labor market, others believed slavery to be an ever-present threat to their freedom. As in the North, this opposition to slavery took a variety of forms and did not necessarily assure sympathy for the slave. Hatred of slavery and the slave frequently became one.

Still, whatever the precise target, worker complaints about slave competition as a threat to their own liberty grew during the antebellum years. True to the proslavery argument, some of these complaints had no abolitionist import and sought only the removal of slaves from specific trades or from artisan work generally. Such petitions bespoke opposition not to slavery but to slaves. Frequently, they explicitly accepted slavery. Slaveholders generally found it easy to deflect these attacks and direct them to the most vulnerable element of the black population, the black

²⁹ Eric Foner, *Free Soil, Free Labor, Free Men: The Ideology of the Republican Party before the Civil War* (New York, 1970); Leonard L. Richards, *Gentlemen of Property and Standing* (New York, 1970); Dawley, *Class and Community*, chaps. 1–4; John Jentz, "The Antislavery Constituency in Jacksonian New York," *Civil War History*, 27 (1981): 101–02; David Montgomery, "Labor and the Republic in Industrial America," *Le Mouvement Social*, no. 111 (1980): 201–15; and Bruce Carlan Levine, "'In the Spirit of 1848': German-Americans and the Fight over Slavery's Expansion" (Ph.D. dissertation, University of Rochester, 1980).

³⁰ According to the historian of Charleston's Irish population, most of that city's Irish workers entered the U.S. in the North and then migrated to Charleston, and others, searching for work, migrated seasonally between Northern and Southern cities; Silver, "A New Look at Old South Urbanization," 145–46.

freemen. Attempts to proscribe free Negroes consistently met with greater success than did attempts to limit the use of slave workers. But some free workers, frustrated by the ability of slaveholders to preserve the widest range of opportunities for employing their valuable property, threatened to strike out at slavery. "In placing the negro in competition with white mechanics," noted a Charleston workingman in the city's leading daily, "you drag down the latter to a level with the former. This is well calculated to breed discontent and hatred on the part of the white mechanic, and make him an enemy of our institution."³¹

The everyday realities and necessities of working-class life may have reinforced disdain for racial bondage and increased sympathy for slaves among free workers, particularly immigrants. In the absence of residential segregation, workers of all sorts lived in close proximity. In Richmond, Frederick Law Olmsted found "a very considerable population of foreign origin," many of them "very dirty German Jews . . . thickly set in the narrowest and meanest streets, which seem otherwise inhabited by negroes."³² Often neighbors practiced a common trade, as did free black carpenter Richard Washington and Irish carpenter George Mahone, who shared a house with their families in Richmond's second ward. Such men probably did not attend the same church, but they may well have shared in the conviviality of back-alley groceries and groggeries scattered throughout every Southern city. There free workers might strike a profitable bargain for some item of slave-stolen merchandise, or a fugitive slave might purchase a set of freedom papers. Much to the disgust of the leaders of Southern society, many white workers did not understand the niceties of Southern race relations or, if they did, did not seem to care. Few workingmen sold slaves liquor, rented them rooms, and aided them in eluding slavery.³³ "Not only free Negroes," complained a Richmond newspaper in 1860, "but low white people can be found who will secret a slave from his master." Other acts of striking generosity punctuated these commonplaces to suggest how closely shared experience might bind workers together. In 1847, the First African Baptist Church of Richmond sent forty dollars overseas to assist victims of the Irish famine, and ten years later that same church donated a small sum to the city's Irish poor.³⁴

³¹ Berlin, *Slaves without Masters*, 229–33, 349–51; *Charleston Courier*, December 7, 1860, as quoted in Michael P. Johnson, "Wealth and Class in Charleston in 1860," in Walter J. Fraser, Jr., et al., eds., *From the Old South to the New: Essays on the Transitional South* (Westport, Conn., 1981), 74, 80 n.

³² Olmsted, *Journey in the Seaboard Slave States*, 55; and Curry, *Free Blacks in Urban America*, 49–80.

³³ The manuscript census schedules for the large Southern cities we have examined indicate that small shopkeepers were disproportionately foreign-born. These shopkeepers usually had started as wage earners, and they often became wage earners following business failure. Richard Wade has found much evidence of a traffic in petty theft between urban slaves and white shopkeepers. According to Earl Niehaus, Irish grocers were arrested for receiving sugar and flour stolen by New Orleans slaves. Liquor, apparently, was often traded for stolen goods. Wade, *Slavery in the Cities*; and Niehaus, *Irish in New Orleans*. The alliance between immigrant shopkeepers and black slaves often was simply a matter of convenience for both. But, with freedom, it became increasingly important and, in some places, served as the basis for the alliance between black and white Republicans. See, for example, the alliance between black editor John P. Mitchell, Jr., and Irish grocer James Bahen in Richmond; Michael P. Chesson, "Richmond's Black Councilmen," in Rabinowitz, *Southern Black Leaders of the Reconstruction Era*, 202–06, 216.

³⁴ *Richmond Daily Dispatch*, December 21, 1860; and Minutes of the First African Baptist Church, March 1847, October 1857, First African Baptist Church, Richmond, Virginia. Frederick Law Olmsted, visiting the Midlothian Coal Mines outside Richmond, recorded an incident that characterizes far more of the interchange between newly arrived immigrants and black slaves than historians have allowed. Olmsted observed, "Not long since, a young English fellow came to the pit, and was put to work with a gang of negroes. One morning, about

Of course, common conditions did not always promote common understanding or mutual respect. Often just the opposite resulted. Shared values and behavior evolved slowly, unevenly, and imperfectly among Southern urban workers during the antebellum years. Conflict among immigrant, native-born, and black workers divided working people, as did internal differences within each group—between British and Irish immigrants, native-born Northern and Southern whites, Catholic and Protestant immigrants, and black freemen and bondsmen. If white workers, immigrant and native, protested black competition and tried to push black craftsmen from their crafts, native-born workers joined the Plug Uglies, Rip Raps, Blood Tubs, and other nativist gangs to oust the immigrants. Slaves sought the protection of their masters and free Negroes their patrons to secure their jobs and protect their persons. The record of this intraclass hostility is full, and the presence of slavery aggravated and enlarged it.³⁵

But, if free workers were pulled in all directions, their allegiance to the slave regime was never firm. Men and women who had fled the landlord-dominated societies of Western Europe were hardly predisposed to sympathize with the planter class. Slavery remained the linchpin of the Southern order, and the relationship of free workers to that institution continued to be ambiguous at best. Many were too newly arrived to understand it, and some found good reason to oppose it. Some foreign-born workers had been schooled in antislavery beliefs in Europe, including British artisans who had observed or participated in the abolitionist debates, and many others had learned to wield the phrase “wage slavery” to their advantage. The defenders of slavery, who sometimes argued that all work should be done by slaves and even that all workers should be slaves, alienated free workers by undervaluing their labor and, at times, slandering their persons. A whiggish Richmond newspaper’s boast that the major advantage of slave labor was its “exclusion of a populace made up of the dregs of Europe” could not have won the approbation of most free workers in that city.³⁶

a week afterwards, twenty or thirty men called on him, and told him that they would allow him fifteen minutes to get out of sight, and if they ever saw him in those parts again they would ‘give him hell.’ They were all armed, and there was nothing for the young fellow to do but to move ‘right off.’

‘What reason did they give him for it?’

‘They did not give him any reason.’

‘But what had he done?’

‘Why I believe they thought he had been too free with the niggers; he wasn’t used to them, you see, and he talked to ‘em free like, and they thought he’d make ‘em think too much of themselves.’” Olmsted, *Journey in the Seaboard Slave States*, 47–48.

³⁵ For hints of intraclass conflict, see William D. Overdyke, *The Know-Nothing Party of the South* (Baton Rouge, La., 1950). But the composition of the nativist movement (its leaders and its followers) awaits careful study. The class composition of nativist political “gangs” in places like Baltimore also needs study. Anti-immigrant political violence involved far more than minor electoral brawls. In the Louisville election day riot of August 1855 (“Bloody Monday”), twenty-two persons were killed, three in four of them foreigners. The election riots in New Orleans in both 1854 and 1856 were nearly as violent. One of them lasted ten days. Afterward, the vote in Irish districts fell by two-thirds. Irish school teachers and policemen lost their jobs. Some Irish persons lost their lives. In 1856, some Irish residents of New Orleans petitioned the Mexican government, seeking land to colonize there. “The initiation of order,” said the *New Orleans Delta* of native American violence, “is accompanied by murder.” See Niehaus, *Irish in New Orleans*, 84–97. For evidence of clientage connections between free blacks and wealthy whites, see Berlin, *Slaves without Masters*, 316–40; and Michael P. Johnson and James L. Roark, “Charleston’s Free Colored Elite and the Secession Crisis,” unpublished essay courtesy of the authors.

³⁶ *Richmond Whig*, n.d., as quoted in Russell B. Nye, *Fettered Freedom* (East Lansing, Mich., 1959), 311; also see *ibid.*, chaps. 7–8. It would be useful to imagine what the immigrant English puddler and Irish drayman in Richmond thought when reading in the *Richmond Examiner* that the nation’s first immigrants had fled “religious

The evolving defense of slavery, which left little room for free workers, suggests the deep distrust in which Southern slaveholders held free workers and their belief that free workers were not reliable allies. No doubt Southern leaders were hypersensitive to all opposition and apt to read antislavery sentiment into the most casual dissent. Still, as the sectional conflict escalated, their conviction deepened that a Southern counterpart to the Northern free soil movement was developing among Southern workingmen, a movement hostile not only to blacks but also to capitalized black labor—meaning slavery. In 1849, after touring Mobile, Savannah, and Augusta, a correspondent of John C. Calhoun observed that these cities had become unsound on the slavery issue and blamed the growing number of foreign-born workers. “The issue of Free Labour against Slave Labour,” he predicted, “will soon be made in the South.” Such concerns multiplied in the 1850s. A Charleston daily declared that alien mechanics were a “curse rather than a blessing to our peculiar institution.” And, when Little Rock artisans protested against competition from free black and slave laborers, the *Arkansas True Democrat* warned that such a “movement, carried to its fullest extent, would abolish slavery in the South. If the mechanic can justly complain of the competition of slave labor, those engaged in every other industrial pursuit can complain of the negro on the farm.”³⁷

The slave masters’ fear of subversion rested partly in the habits of artisanal and ethnic cohesion, which often overlapped and reinforced each other in ways that planters, like Northern capitalists, believed to be hostile to their rule. But slaveholders also feared that enfranchised and politically active white workingmen saw their interests as different from those of the planter class. In this context, the politics of the 1850s can be understood as an attempt by planters not only to counter subversion from without but also subversion from within.

Seeing the seeds of Southern free soilism among urban immigrant workers, the most astute planters resisted the movement to exclude black laborers, slave and sometimes even free. “Drive out negro mechanics and all sorts of operatives from our Cities, and who must take their place?” asked Christopher G. Memminger, the future Confederate secretary of the treasury, in 1849. “The same men who make the cry in the Northern Cities against the tyranny of Capital—there as here would drive before them all who interfere with them—and would soon raise the hue and cry against the Negro, and be hot Abolitionists—and every one of those men would have a vote.”³⁸

The fear of internal subversion laced Southern politics in the decade before the war and created knotty contradictions that not even the most sophisticated Southern politician could comb out. For example, while some planters argued that

and political persecution” but its new immigrants migrated “merely as animals in search of a richer and better pasture,” lacked “moral, intellectual, or religious wants,” and were (“the mass of them”) “sensual, groveling, low-minded agrarians.” Or when the same newspaper reminded its readers that, “while it is far more obvious that negroes should be slaves than whites, for they are fit only to labor, not to direct, yet the principle of slavery is itself right and does not depend upon differences of complexion.” “Slavery black and white,” the *Richmond Examiner* affirmed, “is necessary.”

³⁷ Calhoun, as quoted in Shugg, *Origins of Class Struggle in Louisiana*, 144; *Charleston Standard*, June 18, 1853; and *Little Rock Arkansas True Democrat*, September 29, 1858.

³⁸ Memminger, as quoted in Starobin, *Industrial Slavery in the Old South*, 210.

reopening the African slave trade would secure nonslaveholder loyalty by allowing them to participate directly in slaveownership, others saw the influx of slaves as a means of ridding Southern cities of politically unreliable immigrant workers. Thus, what began as an attempt to unify the white South sharpened internal divisions and left planters and workers further apart than ever. A similar dynamic can be seen operating in the movement to re-enslave free blacks. Radical Southerners believed that re-enslavement provided greater security for the slave regime. Not only would it free the South of another subversive group, but, by selling enslaved free blacks to nonslaveholders, re-enslavement—like the reopening of the slave trade—would also garner nonslaveholder loyalty. Again, opposition came from a variety of quarters, and this time it not only enlarged the gulf between slaveholders and nonslaveholders but also divided the slaveholders among themselves. If some masters maintained that liquidation of the free Negro caste would secure slavery, others believed that the threat of re-enslavement would drive free Negroes out of the South and leave planters even more dependent on white workingmen. In the eyes of these slaveowners, white workers, not free blacks, offered the greatest threat to slavery. Thus, as slaveholders wrestled with the problems of internal division, they enlarged those divisions and also created new fissures in their own ranks. Little wonder the Confederacy came into the world amid contradictory calls for a white man's democracy and open attempts to disenfranchise some whites, particularly immigrant workers.³⁹

FROM THE FIRST SHOT AT SUMTER, Southern leaders remained unsure of working-class loyalties. Just after the 1860 election, writing from New Orleans, politician John Slidell observed that in that city “seven-eighths at least of the vote for Douglas were cast by the Irish and Germans, who are at heart abolitionists.”⁴⁰ While there were few Irish abolitionists among the workers in any Southern city, such doubts about the allegiance of the largely foreign-born work force only intensified with the onset of civil war. Some immigrants found themselves jailed for speaking their minds too freely on the subject of slavery. From New Orleans, the employer of one newly arrived Englishman appealed to the British government to have his employee released from jail; the immigrant worker had been charged with “using language hostile to slaveholding and introducing in the State [Louisiana] books and papers of similar character.” Thirty months later, the British consul at Charleston reported to the Home Office that “labouring men”—immigrant workers surely prominent among them—“are frequently discharged from their employment and subjected to contumely for not taking up arms. They are frequently arrested and sent to gaol, as liable to conscription.” In 1864, the president of an Alabama railroad conveyed to the Confederate secretary of war his suspicions about the

³⁹ Ronald Takaki, *A Pro-Slavery Crusade: The Agitation to Reopen the African Slave Trade* (New York 1971); Berlin, *Slaves without Masters*, chap. 11; Michael P. Johnson, *Toward a Patriarchal Republic: The Secession of Georgia* (Baton Rouge, La., 1977), chap. 5; and Fred Siegal, “Artisans and Immigrants in the Politics of Late Antebellum Georgia,” *Civil War History*, 18 (1981): 221–30.

⁴⁰ Slidell, as quoted in Peyton McGarrv, *Abraham Lincoln and Reconstruction: The Louisiana Experiment* (Princeton, 1978), 56. Also see Johnson, *Toward a Patriarchal Republic*, 97.

skilled founders and puddlers at the huge Selma iron works. He observed that “all of these workmen are foreigners from Europe, or natives of the northern states, the majority being foreigners,” and then made the common complaint: “These men do not feel identified in any great degree with the South and are not imbued with sentiments and feelings calculated to impress them so strongly in favor of our cause, as to induce them to make any great sacrifice of interest or feeling in its behalf.” In spite of the Union noose tightening around the Confederacy, these foreign workers demanded higher wages and abandoned the Selma works even when their demands were met. Perhaps native-born workingmen would have ordered their loyalties in a similar fashion, but the beleaguered railroad executive doubted it.⁴¹

Dissaffected immigrant workers rallied to the Union flag at the first opportunity and, in places like New Orleans, which early fell to the Union army, provided the basis for a Unionist party. Elsewhere, immigrant workers had to wait until the war’s end to demonstrate their political beliefs, but, when given the opportunity, many did so. In 1865, when the Union army occupied Charleston, federal officers promptly recruited two regiments among the loyal natives—one black, one Irish. A Union soldier, parading through Charleston, noted that the crowds who gathered to watch the column move through the city were “chiefly negroes and Irish, and their delight at seeing us was unbounded, the Irish being quite as enthusiastic in the expression of joy as the negroes.”⁴²

Estimates of the extent of antebellum free soilism, wartime disaffection and disloyalty, and postwar Republicanism among the immigrant workers of the urban South require much more refinement before they can go beyond the complaint of Slidell and others. But, whatever free workers thought about slavery, there can be no doubt that slave masters understood the demands of free workers for the elimination of slave competition as more than a conflict of interest with slaveholding; indeed, ultimately they saw it as a conflict of principles. This understanding held enormous importance in the hothouse of antebellum and reconstruction politics. While the full dimensions of this conflict remain to be explored, its partial outcroppings confirm that immigrant workers in the urban South cannot simply be incorporated into the extant understanding of the nature of Southern society, the evolution of slavery, or the character of antebellum politics. Instead, they demand reconsideration of all.

⁴¹ Daniel Godwin to Newlop Ireland, n.d., enclosed in Ireland to Lord Russell, February 19, 1861, Public Record Office, London, Foreign Office 5/793; Consul Walker to Russell, August 21, 1863, *ibid.*, Foreign Office 5/907; and J. W. Lapsley to J. A. Seddon, February 15, 1864, National Archives, Washington, War Department Collection of Confederate Records, Secretary of War, Letters Received, ser. 5, Record Group 109, L-67 1864. In North Carolina cities, urban workers also played a role in the Unionist “Heroes of America.” See William T. Auman and David D. Scarboro, “The Heroes of America in Civil War North Carolina,” *North Carolina Historical Review*, 58 (1981): 350–51.

⁴² McCrary, *Lincoln and Reconstruction*; General Rufus Saxton to E. M. Stanton, March 1, 1865, S-154 (1865), National Archives, Washington, Records of the Adjutant General’s Office, Colored Troops Division, Letters Received, ser. 360, Record Group 94; and *War Letters, 1862–1865, of John Chipman Gray and John Codman Ropes* (Boston, 1927), 459.

The Level and Structure of Slave Prices on Cuban Plantations in the Mid-Nineteenth Century: Some Comparative Perspectives

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IN THE RECENT RESURGENCE OF INTEREST in slavery and slave societies in America, price data have been a fundamental source for historical and economic analyses of the nature of the slave economy. Following the work of Alfred H. Conrad and John R. Meyer on "The Economics of Slavery in the Ante-Bellum South" (1958), scholars have attempted to make inferences about the behavior, expectations, and beliefs of planters through an analysis of the pattern of changes in the price of slaves over time. Slave prices reflected the expectations of planters about a slave's net productivity and the period over which a slave would be productive. The output of the slave, the market value of the goods and services produced, and the value of the goods consumed by the slave all influenced net productivity. The period over which a slave was expected to be productive was determined by the anticipated lifetime of the individual slave and, important for slave societies in the mid-nineteenth century, the expected duration of slavery as a system of labor control.

Despite their importance, surprisingly little systematic price data is available other than the crucial series developed for slave markets in the United States.¹ Only recently have series been generated systematically for other large American slave

All of the Cuban documents used in this article are housed at the Archivo Nacional de Cuba, Fondo "Escribanías" (Judiciales). They were selected by a historical studies group headed by Manuel Moreno Fragonals. The initial data processing was carried out in the Centro de Computación del Poder Popular de la Ciudad de Habana under the technical direction of Ivan Lejardi, whose collaboration and valuable suggestions are greatly appreciated. Further technical assistance was provided by Tito Díaz. Subsequent processing of the data was carried out by the co-authors at the computing centers at Columbia University and the University of Rochester, with the help of James Irwin. Part of this work was financed by the National Science Foundation. We also wish to thank David Brion Davis, Seymour Drescher, David Eltis, Jackie Goggin, Claudia Goldin, Franklin Knight, and Rebecca Scott for comments on an earlier draft of this essay. For permission to use unpublished data we wish to thank, in particular, Eltis, David Galenson, and Stuart B. Schwartz.

¹ The first systematic price series for slaves in the U.S. South from 1795 to 1861 was presented by Ulrich B. Phillips; see his *American Negro Slavery: A Survey of the Supply, Employment, and Control of Negro Labor as Determined by the Plantation Regime* (New York, 1918), 368–75.

societies, such as Brazil and the British West Indies. Similar information is still lacking for other slave economies in the New World. In this context, the major area that has been least studied is the important slave economy of Cuba. In the mid-nineteenth century, Cuba was the third largest slave system in the Western world, and it became the second longest-lasting Western slave regime. This essay aims to provide a systematic analysis of Cuban slave prices in the vital period of plantation growth in the mid-nineteenth century. The analysis also measures price differentials between male and female slaves and between African- and native-born (creole) slaves as well as between slaves with different occupations. In interpreting these data, we also compare Cuban price patterns to those arising from material available for other slave regimes to determine features that were unique to the Cuban plantation system and those in Cuba that were common to most American slave regimes of the nineteenth century.

THIS STUDY OF CUBAN SLAVERY uses the price and demographic data on Cuban plantation slaves gathered by Manuel Moreno Fraginals in the public and private archives of Cuba.² This extraordinary collection covers slave prices on Cuban plantations from the early eighteenth century until the eve of abolition in the late nineteenth century. The most complete set of price data from this extensive collection, which contains values for over forty-two hundred slaves on fourteen plantations covering the years from 1856 to 1863, was analyzed. Not only are the data most complete for this period, but the era itself was also important in the development of slavery in Cuba and elsewhere in the New World.

Several sources are available for studying Cuban slave prices, the most reliable of which for the purposes of this analysis are prices fixed in the appraisal of sugar plantations that were under adjudication for various reasons, such as inheritance disputes and debt collections, and prices fixed in the insurance policies of planters.³ In both cases similar methods were used to set slave prices. When adjudication was necessary, the litigating parties designated by common agreement a group of three experts with experience with slaves and the sugar business. The five individuals then proceeded to make a list of all slaves. The high cost of these proceedings (all examples are from large plantations) and the powerful interests in conflict guaranteed that the set prices were likely to approximate market values. When insurance policies were written, the price was fixed by discussion among three persons: a representative of the insurance company, a doctor, and the slaveowner. If an agreement between the contracting parties was not reached, no policy was

² For an earlier use of these data, see Manuel Moreno Fraginals, "Africa in Cuba: A Quantitative Analysis of the African Population in the Island of Cuba," in Vera Rubin and Arthur Tuden, eds., *Comparative Perspectives on Slavery in New World Plantation Societies* (New York, 1977), 187–201. For other work by Moreno Fraginals using these data, see *El Ingenio: Complejo económico social cubano del azúcar*, 3 vols. (Havana, 1978), and the translation of an earlier version, *The Sugarmill: The Socioeconomic Complex of Sugar in Cuba, 1760–1860* (New York, 1976).

³ Other sources for Cuba include newspaper notices of sale and the thousands of purchase contracts preserved in the Archivo Nacional de Cuba and in various municipal repositories. In studies of other slave societies, probate inventories, plantation listings, compensation claims for slaves convicted of crimes, and tax reports have been used.

written because the insurance payments were based on the price fixed for the slave. To avoid the possibility of later frauds (for example, the substitution of one slave for another of lesser value) both sources gave a detailed description of each slave for purposes of identification.

We preferred these two sources of slave prices for several reasons. They were both prepared using the same method of evaluating the prices of slaves. Because the prices were set by those familiar with the market for slaves and supervised by interested and contesting parties, they tended to be accurate; conflicts of interest prevented marked discrepancies from the market value. In addition, the sources offer supplementary information about each slave and describe distinctive features that help explain unusual prices.

Data for the years 1856, 1859, 1861, and 1863 were used. Although slavery in Cuba lasted until 1886, it was understood by the end of 1867 that the Spanish government was preparing abolition legislation that was to provide compensation to slaveowners.⁴ This legislation was never written, but the Moret Law of 1870 freed slaves over age sixty as well as those born to slave mothers after that year. Because slaveowners assumed that compensation would be related to the ages of their slaves and because they were afraid slaves nearing age sixty who were still useful would be freed, all Cuban sources concerning slave populations after 1868 are biased with respect to age.⁵

To test the reliability of these plantation lists and their representative nature, we compared them to the data on the slave population of western Cuba presented in the census of 1861.⁶ Within the sample from sugar plantations, the slave population was 60.4 percent male, while for all sugar plantations recorded in the census, 64.0 percent of the slave population was male.⁷ As expected, the ratio of males to females among the African-born slaves (2.16 to 1) was considerably higher than that among the creoles (which was almost exactly 1 to 1). In the prime ages (defined as ages eighteen to thirty-nine), the ratio of males to females for Africans (2.64 to 1) exceeded that for creoles (exactly 1 to 1). The higher ratio of African males in the prime ages, relative to those over age thirty-nine, suggests either that African-born

⁴ On the background to the abolition of Cuban slavery, see Arthur F. Corwin, *Spain and the Abolition of Slavery in Cuba, 1817–1886* (Austin, Texas, 1967).

⁵ The anticipated provisions of the law expected after 1867 divided the slaves into two major categories, based on age: slaves younger than age sixty would be subject to a guardianship (similar to the apprenticeship under English abolition law), and those sixty and above would automatically become free. For this reason (and as a means of ensuring an end to the international slave trade), a slave census was taken, and, by law, the Spanish government had no responsibility for recognizing as a slave any black whom the census omitted. Accordingly, a *cedula* ("schedule") for recording the general data of identification was given to each slaveowner for each slave. Unfortunately, that census is lost, but the general data are preserved; even an elementary analysis of the surviving data, however, demonstrates its inaccuracy.

⁶ El Centro de Estadística, *Noticias estadísticas de la isla de Cuba, en 1862* (Havana, 1864). According to this census, the total slave population of Cuba in 1861 was 370,553, of whom 318,775 lived in the western department. For a discussion of the various Cuban censuses, see Kenneth F. Kiple, *Blacks in Colonial Cuba, 1774–1899* (Gainesville, Fla., 1976).

⁷ El Centro de Estadística, *Noticias estadísticas*, "Distribución de la población en los pueblos y fincas de la isla." Male slaves accounted for 60.8 percent of the slave population in the western department. Census schedules for 1861 and account books from about that time for five plantations indicate that the census undercounted children ages one to ten, for whom the sex ratio was probably close to equality. This underrepresentation may have been the result of conscious omission by planters who feared that the census would be used for tax purposes and therefore did not want to include nonworking slaves.

TABLE 1
Sample of Plantations

<i>Date</i>	<i>Name</i>	<i>Number of Slaves</i>
1856	Santa Susana	447
1856	Santisima Trinidad	63
1856	Nueva Empresa	42
1859	Antonia	283
1859	Triunvirato	315
1859	Ojo de Agua	226
1859	Soledad	230
1861	Santa Susana	361
1861	Santisima Trinidad	126
1861	Santa Rosa	375
1861	San José	338
1861	Concepción	291
1861	Santo Domingo	246
1863	Cuatro Pasos (or Concepción)	215
1863	Santa Lutgaria	366
1863	San Silvestre (or Acana)	285

NOTE: With the exception of Nueva Empresa, which produced coffee, all were sugar plantations. Two of the plantations are listed for two different years, and we have treated these as separate observations; duplication is likely for less than 10 percent of all slaves.

females lived longer than males or that the sex composition of the slave trade shifted during the nineteenth century.⁸ The overall population of the sugar plantation sample was 56.6 percent African. The low share of creoles reflects both the importance of African imports in mid-nineteenth-century Cuban slave plantation expansion and the combined impact of either low fertility of female slaves or a high rate of infant and child mortality.⁹ We also compared the prices of slaves with

⁸ The sex ratio of Cuban slave imports may have changed over the course of the nineteenth century. Herbert S. Klein has estimated that about 72 percent of the Africans arriving in Havana from 1790 to 1820 were male; see Klein, *The Middle Passage: Comparative Studies in the Atlantic Slave Trade* (Princeton, 1978), 222–23. British Foreign Office records (FO 84) that David Eltis examined suggest that males accounted for 75 percent of slave imports in the period 1854–63; Eltis, “The Direction and Fluctuation of the Transatlantic Slave Trade, 1844–1867,” paper presented at the annual meeting of the African Studies Association, 1981. Estimates made by Andres Iduate, who compiled data on expeditions to capture runaway slaves in Cuba between 1820 and 1870, indicate that 60 percent of the slaves imported in the 1840s were male. Shifts in the pattern of slave imports alone, therefore, cannot explain the differing sex ratios across age groups. In the British West Indies mortality of African males exceeded that of females, so that, despite the disproportionate sex ratio among slave imports, a more equal sex ratio prevailed among the African-born population by the early nineteenth century. See, for example, B. W. Higman, *Slave Population and Economy in Jamaica, 1807–1834* (Cambridge, 1976), 71–80.

⁹ The ratio of surviving children through age nine to females ages fifteen to forty-nine in our sample was less than three-fifths that for U.S. slaves in the mid-nineteenth century. See Richard H. Steckel, “The Fertility of American Slaves,” in Paul Uselding, ed., *Research in Economic History*, 7 (Greenwich, Conn., 1982): 241. For a discussion that raises still unresolved questions about the Cuban demographic pattern, see Jack Ericson Eblen,

those recorded in various primary sources and the estimates drawn from such sources by other historians. The prices drawn from the plantations are in accord, in level and movement, with those provided by other sources and analyses.¹⁰

Slavery in Cuba presents a rather unusual pattern of development compared with the other major slaveowning areas of the Caribbean and the New World.¹¹ Settled early, Cuba did not greatly expand its use of slave labor and its sugar economy until the end of the eighteenth century. With this late start, Cuba was also the last area in the Caribbean to end slavery; final emancipation came only two years prior to that of Brazil in 1888. During this relatively brief period, Cuban sugar production rose rapidly. The onset of free trade in the last decades of the eighteenth century and the economic collapse of the major sugar-producing area of Saint Domingue following the revolution in the 1790s led to a dramatic increase in Cuban sugar exports. The level of sugar output in the early 1820s was almost four times that of the late 1780s.¹² The decline of output in the British West Indies with the end of slavery there in 1834, the subsequent reduction of British sugar duties in mid-century, and, most importantly, the expansion of the North American market caused sugar output in Cuba to rise again after the 1820s, when Cuba became the world's largest producer of cane sugar. The level of sugar output in the late 1850s was nearly six times the average for 1825–29, and by the early 1870s Cuban sugar production was nearly ten times the level of the late 1820s. During most years between the mid-1850s and the end of the 1870s, Cuba accounted for over one-third of the world's estimated cane sugar production and over one-quarter of total sugar output.¹³

The increase in sugar production was made possible by shifts in the use of the island's slave labor away from previous employment in urban areas and in the production of tobacco and coffee. More important was the expansion of slave imports from Africa in the middle of the nineteenth century. Despite its illegal

"On the Natural Increase of Slave Populations: The Example of the Cuban Black Population, 1775–1900," in Stanley L. Engerman and Eugene D. Genovese, eds., *Race and Slavery in the Western Hemisphere: Quantitative Studies* (Princeton, 1975), 211–47.

¹⁰ The contemporary commentaries examined include those of the *Boletín Comercial de la Habana* and the letters of Juan Poy, owner of the plantation Las Cañas. Also see Hubert H. S. Aimes, *A History of Slavery in Cuba, 1511–1868* (New York, 1907), 267–68; and Roland T. Ely, *Comerciantes cubanos del siglo XIX* (Bogotá, 1961), 121–23. Various references to Cuban slave prices were made by the British consul-general in Havana during this period. Many of these can be found in the *Slave Trade* subject set of British Parliamentary Papers reprinted by the Irish University Press.

¹¹ Among the most complete recent discussions of slavery in Cuba are the works of Manuel Moreno Fraginals cited in note 2, above. Also see Fernando Ortiz, *Los Negros Esclavos* (2d edn., Havana, 1975), and *Cuban Counterpoint: Tobacco and Sugar* (New York, 1947); Herbert S. Klein, *Slavery in the Americas: A Comparative Study of Virginia and Cuba* (Chicago, 1967); Franklin W. Knight, *Slave Society in Cuba during the Nineteenth Century* (Madison, Wisc., 1970); Rebecca J. Scott, *Slave Emancipation and the Transition to Free Labor in Cuba, 1868–1895* (Princeton, forthcoming); Aimes, *History of Slavery in Cuba*; Robert Louis Paquette, "The Conspiracy of La Escalera: Colonial Society and Politics in Cuba in the Age of Revolution" (Ph.D. dissertation, University of Rochester, 1982); and Ramiro Guerra y Sánchez, *Sugar and Society in the Caribbean: An Economic History of Cuban Agriculture* (New Haven, 1964).

¹² Noel Deerr, *The History of Sugar* (London, 1949–50), 131.

¹³ Moreno Fraginals, *El Ingenio*, 3: 35–40, 67–77. Cuban sugar exports to the United States in the 1850s were more than triple those of the 1840s and remained high even during the U.S. Civil War. By the late 1850s the United States received about one-half of all Cuban sugar exports. For an alternative series on Cuban sugar production, showing the same general trends, see Deerr, *History of Sugar*, 131.

nature and the constant pressure of the British to terminate this trade, the import of slaves into Cuba remained high after 1850. After the final closing of the slave trade to Brazil in mid-century, Cuba was the only major area in the New World still receiving slaves from Africa. In the period 1856–63, slave imports into Cuba from Africa averaged over thirteen thousand per year, with a high of twenty-five thousand in 1859 alone.¹⁴

Thus the period 1856 to 1863 was characterized by the last major importations of slaves from Africa. Over one hundred and thirty thousand slaves were imported during the decade 1853–62, which is equivalent to about 35 percent of the number of slaves recorded in the population census of 1861. In addition to slaves, Chinese and Yucatecan contract labor was also imported for plantation work, and between 1853 and 1861 at least forty thousand Chinese landed in Cuba.¹⁵ This was also a period of relatively stable sugar prices despite the considerable growth in sugar production for European and North American markets.¹⁶

The nature of slavery in the Western Hemisphere also changed dramatically during this period. Slavery had ended in the British (1834), Swedish (1847), French (1848), and Danish (1848) West Indies. And in 1863 it ended in the Dutch colonies. More significant, of course, was the outbreak of the U.S. Civil War and the rather precarious position of slavery in the United States by 1863. The persistence of slavery there was by then doubtful, and this might be expected to have influenced the Cubans' belief as to the duration of their own system. Import and slave price data indicate, however, that planters in Cuba did not anticipate an early end to the slave system at the beginning of the 1860s, since they were willing to pay ever higher prices to acquire slaves and continued to import slaves in great numbers.

¹⁴ David R. Murray, *Odious Commerce: Britain, Spain, and the Abolition of the Cuban Slave Trade* (Cambridge, 1980). Murray observed that "the last slave importation into Cuba which British officials there believed had occurred took place in the summer of 1867"; *ibid.*, 324. For estimates of the magnitude of the Cuban slave trade in the nineteenth century, also see David Eltis, "The Direction and Fluctuation of the Transatlantic Slave Trade, 1844–1867," and his forthcoming book; and, for additional data, see Philip D. Curtin, *The Atlantic Slave Trade: A Census* (Madison, Wisc., 1969), chap. 2; David Eltis, "The Direction and Fluctuation of the Atlantic Slave Trade, 1821–1843: A Revision of the 1845 Parliamentary Paper," in Henry A. Gemery and Jan S. Hogendorn, eds., *The Uncommon Market: Essays in the Economic History of the Atlantic Slave Trade* (New York, 1979), 273–301; Klein, *The Middle Passage*, 209–27; and Aimes, *History of Slavery in Cuba*, 269. For recent estimates of the nineteenth-century slave trade, see Paul E. Lovejoy, *Transformations in Slavery: A History of Slavery in Africa* (Cambridge, 1983), 135–58.

¹⁵ A total of about 40,000 Chinese arrived before 1861, according to the *Boletín de Migración* of the time. The census of 1861 shows a Chinese population of 34,828, which seems quite reasonable given the highly skewed sex ratio (almost all the Chinese were male), the high rate of mortality, and the return to China of some laborers who had completed their contract period. The import of Yucatecan labor was quite limited; the census of 1861 shows only 1,046 in Cuba. For detailed discussions of the demographic and economic aspects of the Chinese coolie trade, see Juan Pérez de la Riva, *El Barracón: Esclavitud y capitalismo en Cuba* (Barcelona, 1978). Pérez de la Riva estimated that about 50,000 Chinese migrated to Cuba from 1853 to 1861 and that a total of 124,813 Chinese coolies had landed between 1848 and 1874; *ibid.*, 58. Also see his *Para la historia de las gentes sin historia* (Barcelona, 1976).

¹⁶ For London sugar prices in this period, see Deerr, *The History of Sugar*, 531; and Howard Temperley, *British Antislavery, 1833–1870* (London, 1972), 270–71. For wholesale prices of sugar in the United States, see U.S. Bureau of the Census, *Historical Statistics of the United States: Colonial Times to 1970*, pt. 1 (Washington, 1975): 209; and, for data on unit values of sugar imported into the United States, see U.S. Department of the Treasury, Bureau of Statistics, *The World's Sugar Production and Consumption* (in Monthly Summary of Commerce and Finance, January 1902; House of Representatives, 57th Congress, 1st Sess., Doc. No. 15, pt. 7), 2691–92. The publications of Willett and Gray, New York sugar merchants, also contain an important set of sugar prices.

THE INFORMATION FOR EACH OF THE CUBAN SLAVES for whom we have data in the years 1856, 1859, 1861, and 1863 consists of the name of the plantation where resident and the name, sex, color, age, nationality, traits, stature, moral character, health, occupation, and the price of the slave.¹⁷ To provide the desired homogeneity for comparative purposes, we broke the entries into four categories based on information about health and occupation: healthy and unskilled (2,786); healthy and skilled (752); unhealthy and unskilled (556); and unhealthy and skilled (115). We restricted most of our analysis to the healthy and unskilled category, which was the largest as well as the one that should provide the greatest comparability with

TABLE 2
Average Prime-Age Slave Prices on Cuban Plantations
(in pesos)

	1856	1859	1861	1863
CREOLE MALE	668	1271	1163	914
CREOLE FEMALE	622	1217	1057	830
AFRICAN MALE	628	1212	1125	809
AFRICAN FEMALE	573	1152	1073	725

slave populations elsewhere. Within these four broad classes we then divided slaves into four further categories on the basis of place of origin and sex: creole male; African male; creole female; and African female. In addition, we made separate calculations of prices of slaves at each age for each year within each subcategory.

We based our analysis on two types of calculations. First, we used the prices of prime-age slaves in each of the four years to examine both the movement of overall prices within the period and the relationships between male and female, and African and creole, prices. Second, within each year we computed the ratio of the average price at each age to the prime-age price in the sex and origin category (for example, creole male). Since the structure of prices within each of the years was similar, we averaged the four years (1856, 1859, 1861, and 1863) together when examining the age-price structure. Neither different age ranges for the prime-age

¹⁷ Even when age was recorded, as for creole slaves born on the plantation, it remained a subject for litigation. Consequently, the concept of age should not be understood purely chronologically (as the amount of years lived since birth) but also quantitatively as the estimated physical capability of the slave. For example, some well-preserved old people were assigned ages lower than their chronological ones. Calculated by slave experts, age was estimated each time slaves were appraised or sold, and a slave's chronological age and assigned age must have been related. Nevertheless, for the purpose of broad comparisons of age-price patterns, particularly for creole slaves, the recorded ages should be sufficiently accurate to be useful. Doctors used a special nomenclature to designate the health of slaves. In many instances it is difficult to find the corresponding formulations in present-day medical terminology. Therefore, when we chose slaves for the basic comparisons, we selected only those for whom no unhealthy characteristics had been recorded. The occupation listed was generally the most important job of several that the slave carried out on the plantation. If there was no definite occupation, nothing was written down, or the occupation was designated *campo* ("field laborer").

groups nor different schemes to weight the years affected the results or our interpretations of the age-sex-price pattern.

Sharp changes in the nominal price of slaves between 1856 and 1863 mirrored somewhat the movements in sugar production and slave imports (see Table 2). The price for creole males in the prime ages rose 90 percent from 1856 to 1859—from 668 pesos to 1,271 pesos.¹⁸ These prices then fell somewhat to 1,163 pesos in 1861 and to 914 pesos in 1863. Even with the drop in the price of slaves, the average price of a healthy fieldhand in 1863 remained more than one-third above the 1856 level. Despite the increase in the number of African and Chinese laborers arriving in Cuba, slaveowners were willing to pay higher prices for slave labor, prices that were in excess of those a decade earlier. Indeed, slave prices in 1859 were at the highest level they had yet reached, and even those of 1863 were quite high by historical standards.

The movement of slave prices in Cuba was paralleled elsewhere in the Americas, reflecting the expanding demand in Europe and in North America for slave-produced commodities.¹⁹ At this time, much of the world's traded output of sugar, cotton, coffee, and tobacco was produced by slave labor, and the increasing demand for foodstuffs and textiles enhanced the profitability of slavery wherever it still existed.²⁰ The nominal price of prime-age male fieldhands sold in New Orleans had risen from \$697 in 1850 to \$1,085 in 1856 and reached a peak of \$1,451 in 1860. With the onset of the Civil War the prices declined, falling to \$1,116 in 1862, after which Confederate price inflation and wartime circumstances limit the comparability of price estimates.²¹ In the coffee-producing region of Rio Claro in Brazil, nominal prices of male slaves (ages fifteen to twenty-nine) had risen from 650 milreis in 1850 to 870 in 1852, 1,700 in 1856, and 2,030 in 1860, falling to 1,860 in 1862 but not declining sharply until after 1880.²² In Vassouras, also a

¹⁸ We have neither a general index of prices nor a series of export prices of sugar for Cuba during this period, but the increased nominal price of slaves substantially exceeded the increase in the price of sugar in the United States. See the sources cited in note 16, above.

¹⁹ Economic activity in the United States and England peaked in 1857, but both of these economies recovered rapidly from the cyclical downturn. The economic experience of the United States was, however, affected by the outbreak of the Civil War. See the annual estimates that underly Robert E. Gallman's analysis in "Gross National Product in the United States, 1834–1909," in Dorothy S. Brady, ed., *Output, Employment, and Productivity in the United States after 1800* (New York, 1966), 3–76. Also see J. R. T. Hughes, *Fluctuations in Trade, Industry, and Finance: A Study of British Economic Development, 1850–1860* (Oxford, 1960), 34–68. Sugar prices in 1857 were higher than at any other time from 1824 to 1863; U.S. Bureau of the Census, *Historical Statistics*, pt. 1: 209. Daily prices for New York indicate that the price of sugar in October 1857 was at its highest level in fifty years. Prices did drop sharply in November, but by January 1858 sugar prices had begun to recover rapidly.

²⁰ For data on trade by commodity and by country, see John R. Hanson II, *Trade in Transition: Exports from the Third World, 1840–1900* (New York, 1980), 53–88, 171–81.

²¹ Laurence J. Kotlikoff, "The Structure of Slave Prices in New Orleans, 1804 to 1862," *Economic Inquiry*, 17 (1979): 496–518. (This article is reprinted in Robert William Fogel and Stanley L. Engerman, eds., *Without Consent or Contract: Technical Papers on Slavery* [New York, forthcoming].) Kotlikoff's data are drawn from the New Orleans slave sale records, and, although there are minor differences with the more familiar Phillips series, the general pattern is similar.

²² Warren Dean, *Rio Claro: A Brazilian Plantation System, 1820–1920* (Stanford, 1976), 55. A similar pattern of price increase, plateau, and late decline was found in the coffee-producing region of Campinas; Robert Wayne Slenes, "The Demography and Economics of Brazilian Slavery, 1850–1888" (Ph.D. dissertation, Stanford University, 1975), 183, 253, 234–68. British Foreign Office Consular reports also present extensive data on Brazilian slave prices. Many of the reports are reprinted in Parliamentary Papers. They cover most of the important slave-owning areas and indicate a sharp rise in slave prices in the 1850s, even after the Brazilian slave trade had closed. These data also provide information on the relative prices by sex and by place of origin

Brazilian coffee-producing area, the average price of male and female slaves had risen from about 550 milreis in 1850 to over 1,300 in 1856 and about 1,450 in 1860. Prices there continued to rise until 1880.²³ In the city of Rio de Janeiro the average sales price of all slaves rose from 320 milreis in 1850 to 874 in 1856 and 1,016 in 1860, falling to 797 in 1866—but, again, with no further sharp drop in prices until the 1880s.²⁴ And, in the sugar-producing state of Pernambuco, the average nominal prices of slaves ages twenty to twenty-five rose from 450 milreis in 1852 to 1,200 in 1857 and 1,500 in 1860. Some decline began thereafter, but prices did not return to the nominal values of 1852 until after 1880.²⁵

Thus, for the major plantation regions in the Americas, slave prices increased rapidly during the 1850s. Generally, in those areas where slavery did not end, prices declined slightly in the early 1860s, but slave prices in the mid-1860s remained considerably above those in the years before 1850.²⁶ Given the importance of slave production for the expanding consumer markets in Europe and in North America, the broad similarity of slave price movements in the 1850s in the three remaining major slave powers—Cuba, the U.S. South, and Brazil—is not surprising. In none of these cases do price patterns suggest that the slave system was perceived to be on the edge of collapse or that a permanent decline in profitability was anticipated by slaveowners.²⁷

Another important question is the relative difference in the prices of males and females.²⁸ Average prices for both creole and African female slaves in the prime-

consistent with the pattern found in other sources for Brazil. Slenes prepared a price series for Bahia from this information; "The Demography and Economics of Brazilian Slavery," 182, 215.

²³ Stanley J. Stein, *Vassouras: A Brazilian Coffee County, 1850–1900* (Cambridge, Mass., 1957), 229.

²⁴ Pedro Carvalho de Mello, "The Economics of Labor in Brazilian Coffee Plantations, 1850–1880" (Ph.D. dissertation, University of Chicago, 1977), 50, 40–62. De Mello's data on the prices of slaves, adjusted for changes in the overall level of prices, show a smaller increase, although prices still more than doubled in the 1850s. Prices declined sharply in the 1880s with the expectation of abolition. De Mello pointed to the shifting structure of slave prices by age as the end of slavery was expected. De Mello, "Economics of Labor in Brazilian Coffee Plantations," 181–211. A revised version of this chapter, "Expectation of Abolition and Sanguinity of Coffee Planters in Brazil, 1871–1888," is forthcoming in Fogel and Engerman, *Without Consent or Contract: Technical Papers*. Also see Jaime Reis, "The Impact of Abolitionism in Northeast Brazil: A Quantitative Approach," in Rubin and Tuden, *Comparative Perspectives*, 107–22.

²⁵ Peter L. Eisenberg, *The Sugar Industry in Pernambuco: Modernization without Change, 1840–1910* (Berkeley and Los Angeles, 1974), 153. Eisenberg's real price series shows a smaller increase, but, in general, prices throughout the late 1850s and the 1860s were above the levels of the early 1850s.

²⁶ The same pattern occurred in Ponce, Puerto Rico, a sugar-producing region. The price of male field slaves of working ages rose from 400 pesos in 1855 to about 675 pesos in 1860, before falling to 575 pesos by 1865. José Curet, "About Slavery and the Order of Things: Puerto Rico, 1845–1873," in Manuel Moreno Fraginals et al., eds., *Slavery and Free Labor in the Spanish-Speaking Caribbean: Cuba, Puerto Rico, and the Dominican Republic in the Late Nineteenth Century* (forthcoming).

²⁷ Hubert H. S. Aimes presented slave price data through 1875 that show a somewhat confusing pattern, but none of the post-1860 prices are below those prior to 1850; *History of Slavery in Cuba*, 267–68. The price of slaves would be expected to decline following the passage of the Moret Law of 1870, the abolition of slavery in Puerto Rico in 1873, and the Spanish government's announcement that abolition would be extended to Cuba as soon as the war (which had begun in 1868) ended. By this time, however, relatively few slaves were sold, and thus Aimes's figure could be biased. Slaveowners may have expected slavery to end, and they may have anticipated full compensation for their losses. Based on previous experience elsewhere, however, this is doubtful.

²⁸ The male-female and creole-African differentials shown in Table 2 were less when prices were higher than when prices were lower, although these variations are not dramatic. One unusual pattern appears in 1861: African females carried higher prices than did creole females. We cannot explain this particular observation.

age category were about 90 to 95 percent of those of prime-age males.²⁹ The ratio of male to female Cuban slave prices was not dramatically different from that observed earlier in some of the British West Indies,³⁰ and in the United States, where prices of female slaves were in the range of 80 to 90 percent of those of males. Based on a large sample of probate data for the South in the nineteenth century, Robert William Fogel and Stanley L. Engerman estimated that prices for healthy female fieldhands were about 80 percent of those of males.³¹ In earlier British West Indian evidence concerning the sales of slaves in Barbados by the Royal African Company between 1673 and 1723, prices for females were about 81 percent of those for men.³² Even earlier, on Barbados in 1650, Richard Ligon estimated that the price of women slaves was between 83 and 90 percent of that of the "best male Negroes."³³ Higher male than female prices were also recorded in South America, although in some cases the price for a female slave appears to have been relatively lower than in Cuba. In the Colombian mining zone of the Chocó, female slaves in the prime-age group had an average value in the late eighteenth century of about 94 percent that of males.³⁴ For Bahia, Brazil, prices for adult

²⁹ Comparisons of the price structure for Cuban slaves with those in other slave societies in various time periods are complicated by differences in the classifications of occupation and health and by differences in the age boundaries of the groups compared. The prime ages are defined differently. Some comparisons include all adults, both skilled and unskilled. In some cases small samples or observations based on a limited number of plantations or on the impressions of one or two observers are used. Nevertheless, we believe that such data are of use for broad comparisons and raise interesting issues for further examination. The availability of plantation and probate records in many slave societies should permit broader comparisons when these records are systematically analyzed.

³⁰ Based on figures and discussions in Higman, *Slave Population and Economy in Jamaica*, 187–211. Higman pointed out that "after age twenty males were once again valued more highly, but the difference was small, generally only £10, or less than 7 percent of the total value," and he explained some apparent, larger differentials "in terms of skills of the males"; *ibid.*, 192. (Higman previously noted that "the sexes then seemed to have reached a rough equality during adolescence"—a point that we shall discuss below.) Also see Michael Craton, "Jamaican Slavery," in Engerman and Genovese, *Race and Slavery in the Western Hemisphere*, 278. A listing from 1830 for the "Plantation called Martin Byam's" on Antigua shows female fieldhands in the prime age category valued at 89 percent of the valuation of males; Public Record Office, London, 171/1542 (bundle for Antigua). J. Harry Bennett, Jr., has shown that, on the Codrington plantations on Barbados in 1775, prices for male and female fieldhands were equal, although, with allowances for differences in skill distribution, the value of males was higher (female average values were 89 percent of those of males); Bennett, *Bondsmen and Bishops: Slavery and Apprenticeship on the Codrington Plantations of Barbados, 1710–1838* (Berkeley and Los Angeles, 1958), 13–19.

³¹ Robert William Fogel and Stanley L. Engerman, *Time on the Cross: The Economics of American Negro Slavery* (Boston, 1974), 59–106; and *Time on the Cross—Evidence and Methods: A Supplement* (Boston, 1974), 21–26, 79–85. A small sample of slave sales in several urban areas of the South between 1842 and 1860 (heavily weighted toward the last three years of the period) shows that female prices ranged from 73 to 95 percent of those of males. See Robert Evans, "The Economics of American Negro Slavery," in Universities-National Bureau Committee for Economic Research, *Aspects of Labor Economics* (Princeton, 1962), 225. Kotlikoff, using the New Orleans sales data, estimated that "males sold on average for a 9.1 percent premium relative to females"; "The Structure of Slave Prices," 502. These two estimates probably are affected by their urban origin and by the absence of control for specific ages.

³² David W. Galenson analyzed evidence from the sales of slaves by the Royal African Company in Barbados from 1673 to 1723 and found that the mean value of the annual ratio of women's to men's prices was 0.85 for slaves sold in the first half of the sales auction; Galenson, "The Atlantic Slave Trade and the Barbados Market, 1673–1723," *Journal of Economic History*, 42 (1982): 491–511. Prices varied with the duration of the auctions at which sales occurred; for the full sample, the female-male price ratio was 0.81. No information on the exact age of the slaves is available. Also see Galenson, "The Slave Trade to the English West Indies, 1673–1724," *Economic History Review*, 32 (1979): 241–49.

³³ Richard Ligon, *A True and Exact History of the Island of Barbadoes* (London, 1657), 46; and Vincent T. Harlow, *A History of Barbados, 1625–1685* (Oxford, 1926), 313.

³⁴ William Frederick Sharp, *Slavery on the Spanish Frontier: The Colombian Chocó, 1680–1810* (Norman, Okla., 1976), 203, 120–122. In Colombia "throughout the colonial period female slaves brought prices roughly equivalent to those paid for male slaves"; *ibid.*, 122.

female fieldhands ages fifteen to fifty in the eighteenth and early nineteenth centuries were generally about 70 percent of those of males.³⁵ And, in Rio Claro, in the coffee region of São Paulo, “females were sold on the average for three-quarters of the value of males” in the mid-nineteenth century.³⁶

A pattern of higher male than female prices can be found in several other slave societies. In nineteenth-century Siam the law regulating “the price of liberty for a slave born of a slave in his master’s house” set a value for young adult female slaves ages twenty-one to thirty equal to 86 percent of that of males ages twenty-six to forty.³⁷ In the Edict of Diocletian (A.D. 301), the maximum price for female slaves ages sixteen to forty was five-sixths of that for males.³⁸ In Delphi between 201 B.C. and 1 B.C. adult female slaves paid a price for freedom that was about 80 percent of that paid by males.³⁹ In the ancient Near East in the Neo-Babylonian period the price of a female slave was “somewhat less” than that of a male, while in the Persian period prices were “90 to 120 for a male slave and 60 to 90 shekels for a female slave.”⁴⁰

In the New World female prices occasionally exceeded those of males in urban areas. Pedro de Mello found this pattern for the city of Rio de Janeiro in the second half of the nineteenth century.⁴¹ Similarly, female slaves sold in the city of Lima, Peru, between 1560 and 1650 frequently received prices above those of males, despite fluctuations over time.⁴² Another case of female slave prices exceeding those for males occurred in the city of Seville between 1470 and 1525.⁴³ In examining slavery in Europe in the later Middle Ages, Charles Verlinden found

³⁵ Unpublished data from Bahian sugar plantations between 1710 and 1827, from Stuart B. Schwartz, to appear in his *Sugar, Slaves, and Society in Brazil: The Bahian Recôncavo and Its Environs, 1550–1830* (forthcoming). Also see his “The Manumission of Slaves in Colonial Brazil: Bahia, 1684–1745,” *Hispanic American Historical Review*, 54 (1974): 603–35. The data in his article show a range of average prices of females for manumission between 80 and 90 percent of those of males. Another set of observations for mid-nineteenth-century Bahia shows that, although slave domestics were valued above fieldhands, prices of female fieldhands ages twenty-five to thirty-four averaged 95 percent of those of males; Maria Luiza Marcilio, “The Prices of Slaves in XIXth-Century Brazil: A Quantitative Analysis of the Registration of Slave Sales in Bahia,” in *Studi in Memoria di Federico Melis*, 5 (1978): 83–97.

³⁶ Dean, *Rio Claro: A Brazilian Plantation System*, 58. In Campinas the annual ratio of prices for female slaves to those for male slaves ages fourteen to twenty-eight ranged between 60 and 98 percent from the early 1850s to 1887. The percentage declined from above 80 percent after the mid-1860s. Slenes, “The Demography and Economics of Brazilian Slavery,” 252–58. For an example of male slaves on coffee plantations valued at about 10 percent above female slaves, see note 41, below. More work on Brazilian patterns is needed to reconcile these variations.

³⁷ Akin Rabibhadana, *The Organization of Thai Society in the Early Bangkok Period, 1782–1873*, Cornell University Department of Asian Studies, Southeast Asia Program, Data Paper no. 74 (Ithaca, N.Y., 1969), 106.

³⁸ See the extract in Thomas Weidemann, *Greek and Roman Slavery* (Baltimore, 1981), 105.

³⁹ Keith Hopkins, *Conquerors and Slaves: Sociological Studies in Roman History* (Cambridge, 1978), 159.

⁴⁰ Isaac Mendelsohn, *Slavery in the Ancient Near East* (New York, 1949), 117–18. The most widely cited of premodern cases, which apparently gives the ratios for annual incomes of free persons rather than the capital values of slaves, shows women ages twenty to sixty valued at three-fifths the value of men in an equivalent age group: Leviticus XXVII:3–7. This low relative valuation of females may reflect differences between annual incomes and capital values, and the value of children of female slaves. Nevertheless, the difference between this value structure and that for most other societies is puzzling.

⁴¹ De Mello, “Economics of Labor in Brazilian Coffee Plantations,” 52. “Given the mix of skills of urban slaves, and the relatively high representation in this sample of urban occupations among female slaves vis-à-vis the male slaves, female slave prices tend to be higher than male slave prices”; *ibid.*, 51. In an analysis of fieldhands on coffee plantations in 1873, however, de Mello found that “male slaves were valued on average about 10 percent more than females”; *ibid.*, 169.

⁴² Frederick P. Bowser, *The African Slave in Colonial Peru, 1524–1650* (Stanford, 1974), 342–45.

⁴³ Alfonso Franco Silva, *La Esclavitud en Sevilla y su tierra a fines de la Edad Media* (Seville, 1979), 108, 119.

that "in Spain female slaves were generally cheaper than males, although the opposite was true in most of Italy." Verlinden explained, "Much of the slave manpower in Spain was used in agriculture and industry, whereas in Italy the domestic slave predominated in the cities and therefore more female workers were required."⁴⁴ Several other cases in which the prices of female slaves exceeded those of males occurred in Africa. This might help explain the unbalanced sex ratio among slaves brought to the Americas in the slave trade, since the African price pattern reflected a greater demand for female slaves there and a reduced willingness to send females rather than males across the Atlantic.⁴⁵ Philip D. Curtin observed that in late-eighteenth-century Senegambia "Africans in the hinterland valued women above men," citing a two-to-one ratio in 1690. On the coast, however, prices may have been equal.⁴⁶ Higher prices for females than males in Africa were also recorded in the mid-nineteenth century in the Sokoto Caliphate and in Zanzibar.⁴⁷ Among African slaves in the Ottoman empire in the late nineteenth century "male slaves fetched slightly lower prices than females slaves," and the pattern of "slightly" higher prices for female slaves was also observed in the twentieth century in the Arabian Peninsula.⁴⁸

The patterns of male-female price differentials in various slave societies suggest that both cultural and physiological factors influenced slave prices.⁴⁹ In those societies in which gang labor on large plantations was the basis of the slave system and where physical strength was most critical, females in field work were valued at lower prices than males, although, as will be seen below, the relative value of younger females was enhanced by their earlier physical maturation. In most slave societies occupations were segregated by sex, with craft positions restricted to males and domestic services mainly to females. The greater frequency of males in higher-

⁴⁴ Verlinden, *The Beginnings of Modern Colonization* (Ithaca, N.Y., 1970), 29.

⁴⁵ See Klein, *The Middle Passage*, 240–42; and Philip D. Curtin, *Economic Change in Precolonial Africa: Senegambia in the Era of the Slave Trade* (Madison, Wisc., 1975), 173–77.

⁴⁶ Curtin, *Economic Change in Precolonial Africa*, 176–77. In eighteenth-century Benin in transactions with Europeans, females were sold at 90 to 95 percent of the price of males; A. F. C. Ryder, *Benin and the Europeans, 1485–1897* (New York, 1969), 209–10. Female slaves ages eighteen to twenty-two purchased by the Portuguese in Arguim in 1519–20 sold at a price equal to about 99 percent that of males, while among those ages twenty-three to twenty-seven, females sold at 94 percent of the price of males; A. C. de C. M. Saunders, *A Social History of Black Slaves and Freedmen in Portugal, 1441–1555* (Cambridge, 1982), 24–26.

⁴⁷ David C. Tambo, "The Sokoto Caliphate Slave Trade in the Nineteenth Century," *International Journal of African Historical Studies*, 9 (1976): 187–217; and Allan G. B. Fisher and Humphrey J. Fisher, *Slavery and Muslim Society in Africa: The Institution in Saharan and Sudanic Africa and the Trans-Saharan Trade* (London, 1970), 193–99. Fisher and Fisher's work includes price quotations from Sir Richard F. Burton and Gustav Nachtigal. Concubines generally sold for higher prices than did other females, and girls frequently sold for higher prices than did boys. The latter price structure is consistent with higher male valuation at peak ages and might reflect earlier maturation. See pages 1216–17, below. On the implications of this relative price differential by sex in Africa, also see Lovejoy, *Transformations in Slavery*, 6, 60–65, 139.

⁴⁸ Ehud R. Toledano, *The Ottoman Slave Trade and Its Suppression, 1840–1890* (Princeton, 1982), 65, 62–67. For Arabian prices in the 1930s, see Sir G. Maxwell to Mr. Strang, December 7, 1934, Public Record Office, London, Foreign Office 371/17822; we owe this reference to David Brion Davis.

⁴⁹ The childbearing premium would be expected to be small in most societies, since even in the United States, which had high rates of fertility and survival into adulthood, the value of childbearing explains less than 20 percent of the capital value of a female slave. See Fogel and Engerman, *Time on the Cross: The Economics of American Negro Slavery*, 78–86. Because slave children had positive values in most societies, however, the ability to bear children probably would not have meant a reduction in the value of females. The existence of higher male prices for ages past those at which female childbearing would occur suggests that this latter point cannot account for the differential during childbearing ages.

valued occupations also raised the relative evaluation of adult males, but, given the relative percentages of skilled labor and the magnitude of the skill premium, not by a substantial amount. The more frequent use of slave females in skilled domestic services, however, particularly in urban areas, increased their value. When, as in certain parts of Africa, females were involved more heavily in agricultural production than were males, the price of female slaves sometimes was above that of males.

On Cuban slave plantations, the prices of African slaves of each sex averaged about 88 to 95 percent those of creoles of the same sex. Although it is not possible to determine exactly when these African-born slaves were imported—and thus to know whether they were “post-seasoned”—the price differentials seem smaller than those observed earlier in Jamaica.⁵⁰ It is possible that the somewhat narrower gap in Cuba reflects the fact that most of the plantations in the sample were founded after 1820, mainly with African slaves, and that therefore most of the Africans listed had probably been in Cuba for a number of years. Moreover, given the high level of imports and the late development of Cuban plantations, few of the creole slaves could have been more than the second generation born in Cuba.⁵¹ The value of adjustments to disease environments and the importance planters gave to the process of acculturation make this relatively small differential seem puzzling. If the African-born slaves whose prices are recorded had been imported some years prior to the record, differences in the experience of African- and native-born slaves could become relatively minor.

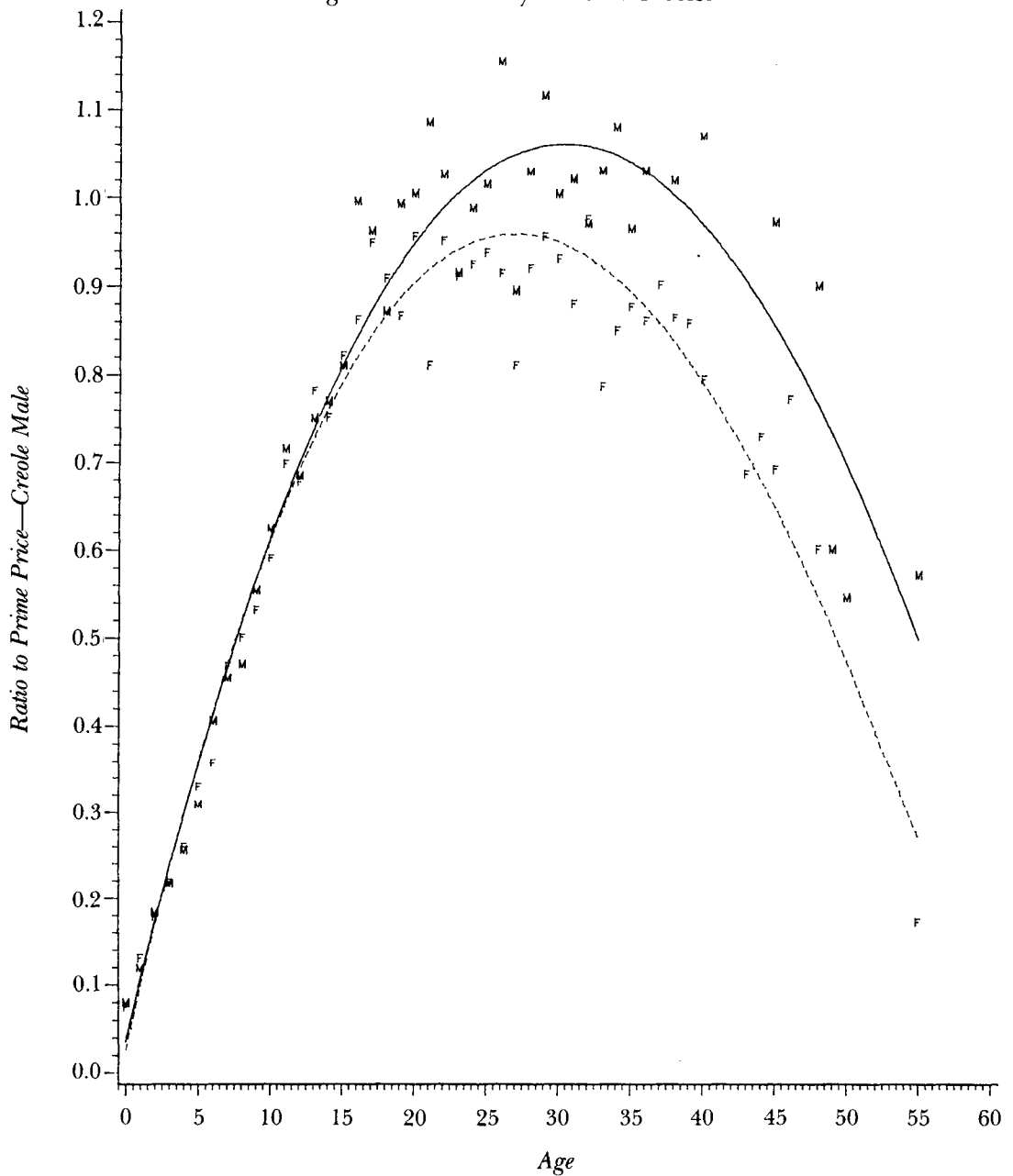
Figures 1 and 2 present estimates of the age-sex-price structures for creoles and for Africans. Creoles and Africans are shown separately, because of differences in the evaluation of slaves by place of origin and also because of problems of comparability outside of the prime-age groups. Many young creoles are included in the sample, but relatively few over age forty. Among Africans this pattern is reversed, with few below age eighteen and a considerable number above age forty. For these reasons, several of the observations made below are drawn either from the data for Africans or from that for creoles, depending on which age group is examined.

Prices for both creole and African males peak at between ages twenty-eight and thirty, similar to the pattern observed for U.S. slaves. For creole males, a price equal to one-half the peak value was reached at about age eight or nine, while among

⁵⁰ Craton, “Jamaican Slavery,” 278. For a smaller differential, see Higman, *Slave Population and Economy in Jamaica*, 192–93. Craton’s comparison is of “new African” and “seasoned slaves.” In general, for the British West Indies, the discussions that appear in Parliamentary Papers before the abolition of the slave trade also suggest that creoles were valued more highly than imported slaves. Based on data in listings for several plantations during the period 1799 to 1831, J. R. Ward estimated “an average value of creoles of similar age and employment” at 20 percent above that of Africans; Ward, “The Profitability of Sugar Planting in the British West Indies, 1650–1834,” *Economic History Review*, 31 (1978): 202–03.

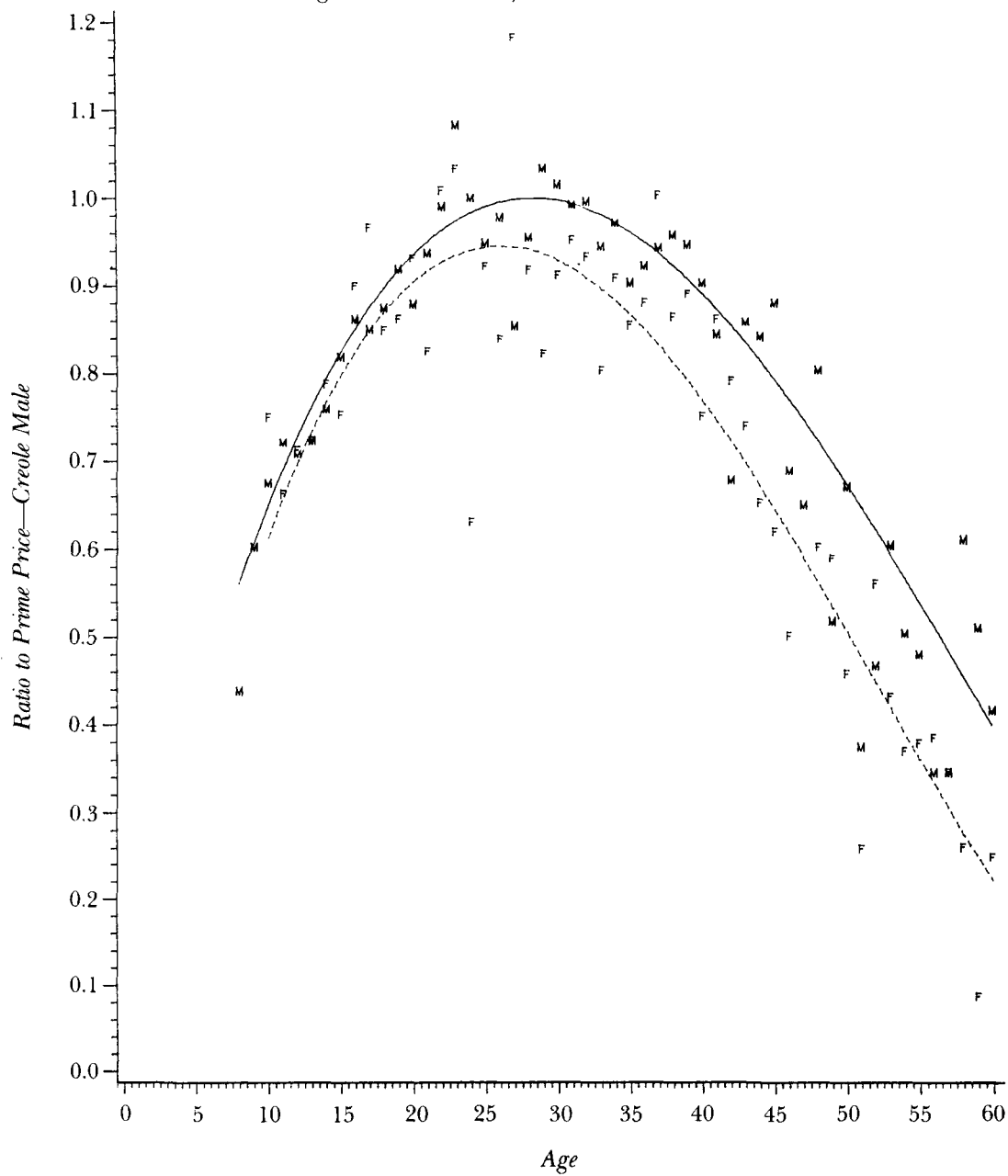
⁵¹ Surprisingly, cases of imported slaves valued at higher prices than native-born slaves are also recorded. In Lima from 1560 to 1650 *bozales* sold at higher prices than *ladinos/criollos*; Bowser, *The African Slave in Colonial Peru*, 342–45. In Rio Claro, no “distinction” was “made between Africans and creoles,” although, apparently, only after the closing of the African slave trade; Dean, *Rio Claro*, 58. The data collected by the British Consuls in the 1850s show a mixed pattern; generally, there was a small premium for creole slaves, although in Bahia in the 1860s African prices exceeded those of creoles; see note 22, above. The premium for Africans was for seasoned slaves, not new arrivals, however, since the slave trade had been ended.

FIGURE 1
Age-Price Profiles by Gender: Creoles



NOTE: The data in Figures 1 and 2 are based on the average of the ratios of the price of a healthy, unskilled slave of a given age relative to the price of a prime-age healthy, unskilled creole male in the

FIGURE 2
Age-Price Profiles by Gender: Africans



year of the observation. To make comparisons clearer, a third degree polynomial was fitted to these data for each of the four categories of slaves.

African males prices fell to about one-half their peak level when slaves were over age fifty; both phenomena are comparable to the pattern for male slaves in the United States. As in the United States, Cuban female slave prices peaked a few years prior to those for male slaves, by age twenty-five. Creole females achieved one-half of the prime price by age seven or eight, while African female prices fell to one-half their prime value after age fifty.⁵²

The relationship between price and age evident for Cuban creoles is similar in one important respect to that found for U.S. slaves—prices for females approached a peak more rapidly than did prices for males.⁵³ Among older slaves (mostly African), female prices decreased somewhat more rapidly than did male prices, a pattern for the sexes similar to that of the United States. The age-sex-price pattern for Cuban creoles permits another useful comparison. The value of an infant, who would not enter field labor for nearly one decade, provides information on the long-term economic and political expectations of slaveowners. The price of slaves under age one ranged from 5 to 10 percent of the price of an unskilled male in the prime-age group, a valuation not dissimilar to that observed in the United States.⁵⁴ This level of Cuban infant prices, even in the early 1860s, suggests that no early end to slavery was anticipated by the island's slaveowners.

In general, the pattern of prices by age and sex for the Cuban slaves does not differ dramatically from the patterns of slave prices in the United States, in Jamaica, and in Brazil. Peak prices were achieved at approximately the same ages.⁵⁵ The similar patterns of relative price increase with age by sex reflect the general earlier physical maturation of females.⁵⁶ Likewise, female prices declined more rapidly after the peak ages than did those of males. Lacking adequate data on Cuban

⁵² For the U.S. data referred to in this and the next paragraph, see Fogel and Engerman, *Time on the Cross: The Economics of American Negro Slavery*, 67–86.

⁵³ A similar pattern is evident for nineteenth-century Brazil, both for the Campinas data that Slenes presented and the Rio de Janeiro data that de Mello collected; Slenes, "The Demography and Economics of Brazilian Slavery," 260; and de Mello, "Economics of Labor in Brazilian Coffee Plantations," 175. A similar pattern also applies to eighteenth-century Colombia; Sharp, *Slavery on the Spanish Frontier*, 203.

⁵⁴ Roughly similar ratios were found to apply to several other slave societies. For Colombia, Sharp's data suggest that "slave children only several months old" were worth 5 to 10 percent of peak prices; *Slavery on the Spanish Frontier*, 120–26, 203. For Barbados, Bennett estimated infant prices at about 8 percent of the peak for fieldhands; *Bondsmen and Bishops*, 13–19. Both sets of data suggest that children's value equaled about one-half that of prime-age males before the child slaves reached the age of ten. Higman presented a similar ratio of infant to peak prices; Higman, *Slave Population and Economy in Jamaica*, 187–211. For Rio Claro, Dean noted that "children at age two were sold for about a tenth of the adult price; by age eight they were valued at about half"; *Rio Claro*, 58. A price pattern with some peculiarities is shown by the tariff of slave prices in Venezuela, used to set the prices of manumissions between 1821 and 1854. Slaves age 8 days were valued at one-sixth of the prime price, and slaves age one at one-third, although one-half of the prime price was reached at age eight (and again after age fifty-three). These schedules do not differentiate slaves by sex. See John V. Lombardi, *The Decline and Abolition of Negro Slavery in Venezuela, 1820–1854* (Westport, Conn., 1971), 157.

⁵⁵ Higman, *Slave Population and Economy in Jamaica*, 192; Slenes, "The Demography and Economics of Brazilian Slavery," 260; and de Mello, "Economics of Labor in Brazilian Coffee Plantations," 155. For the eighteenth-century Chocó, Sharp's data suggest a price plateau from ages fifteen to thirty-nine for both males and females; *Slavery on the Spanish Frontier*, 203. On a Mexican sugar hacienda from 1596 to 1754 slaves reached peak prices at age twenty-five, and their prices declined after age forty-five; Ward Barrett, *The Sugar Hacienda of the Marqueses Del Valle* (Minneapolis, 1970), 83, 97–98. Barrett also pointed out that, "in general, males were more valuable than females"; *ibid.*, 98.

⁵⁶ On earlier female maturation, see Phyllis B. Evelyn and J. M. Tanner, *Worldwide Variation in Human Growth* (Cambridge, 1976), 25–31, 88–89, 98, 137–39. Also see Tanner, *Foetus into Man* (Cambridge, Mass., 1978), 60–77.

fertility patterns, we are unable to determine what proportion of the value of females is attributable to field work and what to their child-bearing capacity, and thus we cannot adequately account for the different rates of price decline for males and females.⁵⁷ Despite differences in fertility and mortality patterns, peak female prices were achieved at roughly similar ages in both the United States and Cuba.⁵⁸

A final noteworthy characteristic of Cuban slave prices is the premium placed on artisanal and other skills. This comparison is restricted to males since, except for domestic duties, Cuban slaveowners (like slaveowners throughout most of the Americas) did not assign women to these occupations. The premium for skilled and semi-skilled males averaged about 10 to 20 percent. The frequency of skilled occupations increased with age, and, at a given age, a greater percentage of the creoles than the Africans were skilled. Nevertheless, because of divergences in age structure between creoles and Africans, more Africans than creoles were trained in skilled occupations, even among machinists.

The technology of sugar production and the nature and meaning of some occupations were transformed in the mid-nineteenth century.⁵⁹ Until about 1840, sugar technology was relatively simple. Because the skills could be acquired quickly, the magnitude of any premium for skilled workers was limited.⁶⁰ In the middle of the century, however, technical developments accelerated, and the adoption of new and costly machinery became widespread. To operate these machines, plantation owners used three sources of labor: salaried workers, Chinese contract labor, and slaves trained for skilled work. All three solutions to the labor problem were pursued, and the availability of nonslave labor sources limited both the number of slaves who were taught new skills and the premiums estimated for skilled slaves. Until further study is undertaken, it is difficult to know whether the premiums for skilled male slaves estimated for the years from 1856 to 1863 can be applied to earlier or later periods.

SEVERAL IMPORTANT CONCLUSIONS ABOUT THE CUBAN SLAVE REGIME as it functioned in the nineteenth century emerge from this survey of Cuban slave prices in the 1850s and 1860s and the comparison of these data with those for slave prices in other regions and times. First, planters did not anticipate an early end to slavery. Despite the abolition of the slave trade to Brazil in the 1850s and of that to Cuba in the 1860s and the ending of the largest of the New World slave regimes by the Civil

⁵⁷ For such a decomposition for the United States, see Fogel and Engerman, *Time on the Cross: The Economics of American Negro Slavery*, 78–86.

⁵⁸ Some other, rougher comparisons of the age-sex-price structure are possible for earlier periods in Europe. In late medieval Seville, prices of slaves peaked between the ages of sixteen and twenty-five and fell to one-half of the peak level after age thirty-six; Silva, *La Esclavitud en Sevilla*, 116–18, 108. In late fourteenth-century Florence, female slave prices were highest between ages fifteen and twenty-five, while among teen-agers boys apparently sold at lower prices than did girls; Iris Origo, "The Domestic Enemy: The Eastern Slaves in Tuscany in the Fourteenth and Fifteenth Centuries," *Speculum*, 30 (1955): 336.

⁵⁹ The discussion in this paragraph is based on Moreno Fraginals, *El Ingenio*.

⁶⁰ In the plantation sample the most frequent skill was *carretero* ("cartman"), an occupation requiring more ability than a fieldhand but easily learned and, therefore, having only a relatively small premium above field labor.

War in the United States, Cuban planters (like those in Brazil) still believed that slavery would continue without serious interruption, and they supported their assumption by paying ever higher prices to acquire slaves. Thus, the price data offer a fascinating insight into the perceptions of the slaveowners in Cuba and elsewhere in the Americas—perceptions of reality that were soon to be seen as extremely distorted in the light of subsequent political developments.

Second, our analysis of the price differentials by age, sex, and origin shows some surprisingly consistent patterns among various slave societies. Rural plantation regimes generally valued prime-age healthy, unskilled women 5 to 20 percent above men. This differential was lower at younger ages. These patterns suggest differences in rates of physical maturation and, possibly, relative physical strength. Male slaves in the Americas had a higher value on plantations than did females in unskilled work. Most of the highly valued occupations on plantations were restricted to males. In urban domestic slavery, however, female slaves were often valued more highly than males. The pattern of higher prices for males than for females prevalent on plantations in the New World was reversed in parts of Africa, even during the period of the Atlantic slave trade. African demands for women to work in agriculture and, possibly, as wives (in some cases as producers of free children) may have made women more attractive than men as slave. Finally, comparisons of African-born and creole slaves in Cuba points to a relatively small difference in prices. This suggests that by the mid-nineteenth century the process of “seasoning,” or adjusting to local climate and diseases, and the impact of acculturation were of limited significance in determining slave prices.

Slave prices are useful not only for estimating the profitability of slavery but also for information on planter expectations and on their perceptions of their chattels. Slave price data can provide important information for understanding the past, and the wide availability of such materials should permit further extension of the geographic and temporal scope of our knowledge.

Review Essay
History as Current Events:
Recent Works on the German Revolution of 1848

DONALD J. MATTHEISEN

NEARLY THIRTY YEARS AGO IN THIS JOURNAL, Theodore S. Hamerow surveyed the traditional historiography of the German revolution of 1848 and found it wanting. He pointed out that each generation of German historians since 1848 had been preoccupied in its own way with the same problems that faced the Forty-eighters—the unity of the nation and its constitutional form. The result was a time-bound and parochial historiography that often misrepresented the past for present political purposes. The Forty-eighters were scorned by Karl Marx and the “school of the left” as half-hearted revolutionaries and by Heinrich von Sybel and Heinrich von Treitschke of the “Prussian school” as inept bunglers whose goals could only be attained by Bismarck’s realistic methods. Later in the century, some National Liberal historians like Erich Marcks and Max Lenz acquired a better appreciation for the Forty-eighters after discovering that the Prussian-German empire owed something to them after all. During the Weimar Republic, democratically inclined members of the craft, above all Veit Valentin, unearthed respectable republican traditions in 1848 that could help legitimize the new regime. For the Nazis, however, 1848 was just another chapter in the age-old Jewish conspiracy against Germany; at the same time, non-German historians (Lewis Namier and A. J. P. Taylor, for example) were developing an anti-Nazi, even anti-German, “revisionist” school of interpretation, in which the roots of Nazism itself were detected in the “seedbed” of 1848. In short, Hamerow found that

The study of the Revolution has suffered from the curse of contemporaneity, from the tendency to interpret it in the light of developments subsequent to it and very frequently irrelevant to it. . . . The tendency of the present to distort the past and force it into its own formulas is nowhere more apparent than in the historiography of the German Revolution of 1848.¹

But Hamerow felt that the Second World War had finally brought an era of ideologically biased historical interpretation to an end. “The truth is,” he wrote,

This essay is an expanded version of a paper read at the Regional Conference of Hudson Valley Colleges and Universities, held on November 7, 1981.

¹ Hamerow, “History and the German Revolution of 1848,” *AHR*, 60 (1954–55): 27–28.

“that neither East nor West [Germany] possesses the intellectual zest or even the interest to present a new interpretation of 1848. The Revolution is no longer the vital problem in German politics which it was for almost a hundred years.” To Hamerow this decline in interest offered an opportunity. For the first time the revolution could be investigated dispassionately, free from the distortions of contemporary politics. The way was open for historians to follow the suggestion advanced by Erich Brandenburg at the turn of the century: that historians study the social factors influencing the upheavals of 1848. Social history, “a rich field for original research,” could provide a new and more authentic interpretation. Social history would enable us finally to answer such questions as “why the German peasantry revolted, what it wanted, and what it got,” to describe the “conflicting interests of the new factory proletariat and the older system of artisan guilds,” to “discuss the political and economic demands of the liberal middle class and interpret constitutional reform in the light of these demands,” and to “analyze the social policies of the conservative aristocracy and explain its eventual success on the basis of those policies.” In sum, Hamerow called for a new history of the revolution that would come closer to the reality of 1848 because it would be based on foundations firmer than ideological expediency.²

Hamerow’s expectation has been fulfilled only in part. During the past generation, German historians, in company with those of other countries, have become increasingly conscious of social and economic influences on human action. The history of 1848 has accordingly acquired a deeper social dimension. Hamerow himself has contributed a magisterial reinterpretation not only of the revolution of 1848 but also of the whole era of German unification, a reinterpretation based solidly on his profound understanding of this social dimension.³ In this respect, Hamerow has been both seminal and prophetic. But his hope for a new age of unbiased scholarship on 1848 has been disappointed, as was probably inevitable. To be sure, as Hamerow foresaw, 1848 is no longer the central issue it once was. In recent years historians have shifted their attention more toward the period of the Bismarckian and Wilhelmine empire, concentrating on such questions as the social and political effects of industrialization, the fusion of the old and the new social elites, the techniques of their domination over the lower class, and the growth of social radicalism. But 1848 is not ignored, nor can it be, for it was either a starting or turning point for a number of significant developments—such as national unification, organized politics, and the labor movement—whose eventual shape the revolution helped to determine. Even now, furthermore, the event is still too sensitive to escape partisan treatment, for Germans are no more immune than the rest of us to the human need continually to bring our understanding of the past into alignment with our understanding of the present. For that reason, as Carl L.

² *Ibid.*, 39, 44.

³ Hamerow, *Restoration, Revolution, Reaction: Economics and Politics in Germany, 1815–1871* (Princeton, 1958), *The Social Foundations of German Unification, 1858–1871: Ideas and Institutions* (Princeton, 1969), and *The Social Foundations of German Unification, 1858–1871: Struggles and Accomplishments* (Princeton, 1972). It does not detract from Hamerow’s achievement to note that his works are fine examples of engaged and partisan history.

Becker said more than a generation ago, "history has to be rewritten by each generation."⁴

Historical interpretations of 1848 have always been politically partisan, and, in spite of continual modification, the partisan lines of interpretation have been amazingly durable over time. In a recent historiographical study, Franzjörg Baumgart described four major schools that arose contemporaneously with the revolution itself: the conservative, liberal, radical, and socialist interpretations. The conservative view found nothing fundamentally wrong with pre-March Germany and blamed the revolution on outside agitators, either rootless and malcontent intellectuals or actual foreigners, usually French or Polish, who took advantage of an economic crisis to mislead a basically loyal and conservative body of contented subjects. All the excitement and rioting and bloodshed were totally without justification. The liberals, of course, argued that something was wrong in Germany, that the spirit of the times called for progressive reform (that is, for national unity and a constitution), and that the revolution should have been pre-empted by timely reforms. When revolution did break out, the liberal leaders tried to mediate between the old authorities and the violent mob, but in vain. Once the revolt began to falter, the conservative monarchs could not be forced to make accommodations. But the defeat of the revolution did not remove the need for liberal reform—only postponed it. Reform would come eventually, one way or another.⁵

The radicals took yet a third view of the revolution. To them it was not just an ugly outbreak of mob violence that had to be fended off. Rather, it was a wholly legitimate attempt by the people to abolish an oppressive order and to replace it with a democratic republic in which all Germans could participate fully, freely, and decisively in the affairs of the nation. The radicals blamed the failure of this uprising on the cowardice of the liberal bourgeoisie, which had seized control of the movement early on and should have led the struggle against the old authorities. Instead, the German bourgeoisie had abused its position of leadership by calming the masses with deceitful promises of reform and then treacherously delivering them back into bondage. In this way the German bourgeoisie had "betrayed" the revolution, which otherwise might well have succeeded. The socialists, taking still another position, agreed with the radicals' analysis of the events of 1848 but felt that the radical vision of the future was much too limited. The real revolution must be more than just a political upheaval: as a response to the developing social problems of industrialization, it required the overthrow not only of the old political order but also of the existing system of property relations. In his study Baumgart showed that these four different lines of interpretation persisted for two generations, developing along parallel tracks but retaining their separate identities.⁶ Although his work carries the story only to 1914, Baumgart's four models are still useful for an

⁴ Becker to William E. Dodd, January 27, 1932, in Michael Kammen, ed., *What Is the Good of History?* *Selected Letters of Carl L. Becker, 1900–1945* (Ithaca, N.Y., 1973), 157. This letter summarizes the essential thrust of his famous AHA presidential address, "Everyman His Own Historian," *AHR*, 37 (1931–32): 221–36.

⁵ Baumgart, *Die verdrängte Revolution: Darstellung und Bewertung der Revolution von 1848 in der deutschen Geschichtsschreibung vor dem ersten Weltkrieg* (Düsseldorf, 1976).

⁶ *Ibid.*

understanding of works published since that date. Virtually every post–World War I work on the subject has as a central motif the elaboration of one of these four basic explanations. The models have undergone some changes, to be sure, but they retain their fundamental outlines. Contrary to Hamerow's expectation, our understanding of the German revolution of 1848 is still hostage to politico-ideological squabbles as old as the revolution itself.

THE CONSERVATIVE POSITION has naturally undergone the most change, primarily because its chief patrons, the monarchists, have almost disappeared. That generation of historians who were born and educated during the second Reich and continued to extol its virtues during the Weimar period has passed away. To defend the monarchy when restoration is no longer even discussed is difficult. Yet some of the old arguments reappeared during the Nazi era, when now-forgotten historians resurrected the issue of outside agitators by attributing the revolution to Jewish conspirators as well as to French and Polish revolutionaries.⁷ Namier, in a famous paper given during the war, revived the "rootless intellectuals" thesis and implied that the revolution was somehow responsible for Adolf Hitler.⁸ And Hans Joachim Schoeps's nostalgia for old Prussia has produced a deeply sympathetic treatment of the Christian German advisers of Frederick William IV.⁹ But, on the whole, the conservative interpretation lacks present-day advocates and, for the moment at least, is in eclipse.

The socialist interpretation has had a far more successful career. The subject has an obvious appeal to the historians of communist East Germany: Marx and Engels were active participants in 1848, and, for the first time in history, the German working class acted as an organized political force. This is an irresistible combination. The revolution occupies a central position in the officially sponsored history of East Germany, which relates events of that year to crucial points of dogma. The most important article of faith is that in 1848 the time was ripe for the German bourgeoisie to perform its "historical task": to seize power for itself by overthrowing the "half-feudal aristocratic-monarchical reaction." (The word "reaction" generally receives this full complement of adjectival modifiers.) The old regime, according to this interpretation, was in fact much weaker than it seemed, because Germany, owing to a long period of creative adaptation to the market by Junkers who followed the so-called Prussian path to capitalism, was already well along the road to capitalist development. To be sure, the bourgeoisie needed allies. The petite bourgeoisie of craftsmen and artisans was available and so, too, were the workers. These subordinate classes shared with the bourgeoisie the need to overthrow the feudal order to make way for their own development. Together, these three forces could have done the job. But the bourgeoisie "betrayed" the revolution by

⁷ For a discussion of the Nazi historiography of the revolution, see Kurt Schwerin, "The Revolution of 1848 and the German Historians" (PhD dissertation, Columbia University, 1955), 417–24.

⁸ Namier, *1848: Revolution of the Intellectuals* (Oxford, 1946). Namier's book is a revision and expansion of his lecture by the same title, which he delivered as the Raleigh Lecture on History for 1944.

⁹ Schoeps, *Das andere Preussen: Konservative Gestalten und Probleme im Zeitalter Friedrich Wilhelm IV* (3d edn., Berlin, 1964), esp. chap. 1.

abandoning its natural allies and actually joining the reactionaries against them. This historic betrayal defeated the revolution. But that does not mean that the revolution bore no fruit; on the contrary, it was a seminal event for working-class consciousness. The workers' political experience in 1848 gave them a sense of their power and a realization of the solidarity of their interests. Owing largely to the leadership of Marx and Engels, this working-class consciousness turned away from mere reformism and trade unionism and adopted a true revolutionary direction. This development was the real seedbed of revolutionary socialism in Germany. The German Democratic Republic is, of course, the full-grown plant.¹⁰ A major function of this socialist version of history is to legitimize the East German regime by showing it to be the fulfillment of the historic struggles of the working class. Its usefulness for this purpose guarantees it a certain currency for the indefinite future.

Naturally, the German Federal Republic, as a parliamentary democracy, can hardly sponsor an official school of history. And yet it too needs the legitimacy that history can provide. The consequences of Germany's division through military defeat in 1945 and the Cold War have narrowed the range of legitimizing historical interpretations. The loss of East Prussia and eastern Pomerania to Poland and of western Pomerania, Brandenburg, and Mecklenburg to East Germany has deprived the Bonn regime of the geographical and economic base for traditional Prussian conservatism. (Ironically, Bavarian Catholicism has taken the place of Prussian Protestantism as the center for what passes as traditional conservatism in West Germany.) Since the Cold War has divided Germany ideologically as well as geographically, the East Germans have gained a near monopoly on the orthodox (that is, Marxist-Leninist) socialist version of history, which has no notable protagonists in West Germany.¹¹ Through the consequences of the Nazi regime and World War II, West Germany has virtually lost two of its traditional historical interpretations, but the liberal and radical interpretations of 1848 have survived in modified form. While each of those interpretations is consistent with their common, contemporary milieu, they present rival visions of the present and future. The lack of historiographic consensus on a variety of subjects is symptomatic of the unquiet

¹⁰ For a discussion of East German historiography on 1848, see two good bibliographical articles by Western scholars: Andreas Dorpalen, "Die Revolution von 1848 in der Geschichtsschreibung der DDR," *Historische Zeitschrift*, 120 (1970): 324–68; and Günter Wollstein, "1848—Streit um das Erbe (I)," *Neue politische Literatur*, 20 (1975): 491–507. The East Germans have provided a résumé of their general interpretation in the form of "theses" for the Fifth Historical Congress of the GDR, printed in *Zeitschrift für Geschichtswissenschaft*, 20 (1972): 1245–63. For some excellent East German discussions of works written on both sides of the border, see Walter Schmidt, "Zur historischen Stellung der 48er Revolution," in Horst Bartel and Ernst Engelberg, eds., *Die grosspreussische-militärische Reichsgründung*, 1 (East Berlin, 1971): 1–23; Helmut Bleiber, "Bourgeoisie und bürgerliche Umwälzung in Deutschland: Zum Stand und zu Problemen der Forschung," *Zeitschrift für Geschichtswissenschaft*, 25 (1977): 305–32; and Gerhard Lozek et al., *Unbewältigte Vergangenheit: Kritik der bürgerlichen Geschichtsschreibung in der BRD* (3d edn., East Berlin, 1977). Günther Heydemann has discussed the relationship between the state and the historical profession in East Germany; see Heydemann, *Geschichtswissenschaft im geteilten Deutschland* (Frankfurt a/M., 1980), 137–234.

¹¹ But there are Marxian interpreters of the revolution outside of the profession as narrowly defined. The 125th anniversary of the revolution saw several Marxist-oriented cultural events in West Germany, including museum exhibits in Berlin and Frankfurt and a television program on Westdeutscher Rundfunk. Thomas Nipperdey's essay, "Kritik oder Objektivität? Zur Beurteilung der Revolution von 1848," though without footnotes, seems to be critical of these efforts; Nipperdey, *Gesellschaft, Kultur, Theorie* (Göttingen, 1976), 259–78.

condition of contemporary West Germany, a country generally said to be in the middle of an identity crisis. Among historians, the most prominent controversies have concerned the “primacy” of domestic or foreign policy, whether German development was a *Sonderweg* (“a unique national phenomenon”), and the extent of German responsibility for World War I, but they have been fought out with new sources and new weapons. Just as Fritz Fischer and Hans-Ulrich Wehler resurrected old disagreements about the Second Empire and recast them in present-day terms,¹² so the ancient arguments about the revolution of 1848 have been dusted off and given a modern vocabulary. Seen in this perspective, the recent works on 1848 fall into the well-worn pattern of German historiographic tradition.

The liberal version has predominated in academic circles since the end of the nineteenth century. It is characterized by an appreciation for the Frankfurt Parliament or, rather, for the parliament’s moderate members—the followers of Heinrich von Gagern, who tried to steer the revolution on a course of prudent and reasonable compromise between extremes of left and right. In this view, the moderates wanted national unification and as much liberal reform as was practicable under the circumstances, but they were also realistic men. They understood that creating a large, centralized German empire would have called forth a coalition of hostile states and led to a general war, which Germany would certainly have lost. So they insisted on an extremely federalized structure and, eventually, on a modest *Kleindeutschland* that excluded the Austrian empire. Representing the upper bourgeoisie, the moderates were naturally not attracted to the ultrademocratic notions of the radical left. Being German as well as bourgeois, they had an appreciation for the blessings of a strong monarchy. But, even if they had been more favorably inclined toward democracy, they knew that democratic schemes were doomed in the Germany of 1848. The liberal explanation argues that the democratic forces were too weak and the old regime, in spite of its momentary paralysis, was still too strong. The inevitable result of trying to install a democratic system would have been a bloody civil war. Instead, the moderates steered away from democratic excesses and cleaved to a centrist line, hoping to draw enough support from both sides to create a consensus. They failed, primarily because the demagogues of the democratic left refused to compromise. The radicals, by continually stirring up civil disorders, eventually frightened most German *Bürger* into a reactionary mood and destroyed whatever slim chance for conciliation remained. But, even though they failed, the moderates have been praised for their humane and liberal values, their clear-minded understanding of the situation, and their courage in attempting to establish a liberal regime before conditions were truly ripe for it. Their place in the pantheon of national heroes is secure. A clear line of sanctifying and legitimizing tradition connects the Federal Republic with this group of prudent, moderate, humane, liberal, and, above all, antirevolutionary gentlemen of 1848.¹³

¹² Fischer took up the old question of “war guilt” for World War I, while Wehler renewed the critical insights of Eckart Kehr that date from the 1920s; Fischer, *Griff nach der Weltmacht: Die Kriegszielpolitik des Kaiserlichen Deutschland, 1914/18* (Düsseldorf, 1961); and Wehler, *Das deutsche Kaiserreich* (Göttingen, 1973).

¹³ For a good discussion of the turn-of-the-century historians, see Schwerin, “The Revolution of 1848 and the German Historians,” esp. chap. 4. During the Nazi era, most liberal historians turned their attention to the national rather than the liberal aspirations of the Forty-eighters, since that approach was much more

PRUDENCE, CAUTION, AND MODERATION were exemplary virtues for the 1950s, when Konrad Adenauer's campaign slogan of "No experiments" was a sure-fire vote-getter. But the age of the social-liberal coalition required a different sort of historical precursor. Hence, it has now been discovered that the cautious liberals of 1848 were, within certain understandable limits, actually very democratic. That is the main point of Baumgart's *Die verdrängte Revolution*, whose classification of historiographic biases I have adopted. Of the four basic interpretations, Baumgart was mainly interested in the development of the liberal one. His thesis is that in the year 1848 liberals were really very favorably disposed toward democracy. After all, they introduced parliamentary government at Frankfurt and hammered out a constitution for Germany that contained such democratic features as universal manhood suffrage, the secret ballot, and a suspensive rather than absolute veto for the monarch. At one point in the parliamentary proceedings Gagern pronounced himself in favor of "national sovereignty," and at that moment, Baumgart claimed, he was truly committed to that basically democratic principle.¹⁴

The moderates expected, however, to have their generously democratic sentiments rewarded by universal national applause and the winding down of the revolution. Instead, the uproar continued. Two deputies of the Frankfurt Parliament were murdered by an angry mob, and, when the Prussian king finally turned down the offer of an imperial crown, armed insurrections actually broke out in several parts of the country. These and similarly shocking events soon frightened the old liberals out of their initial democratic sympathies. The histories that several of their leaders later wrote are highly colored by this trauma. In retrospect, their accounts transformed the popular uprising of March 1848 into a senseless mutiny; its radical instigators, previously their collaborators in drafting the constitution, now became dangerous men. Later still, their new-found antirevolutionary and antidemocratic views were confirmed when the strong man Bismarck showed them that collaboration with the authorities was, after all, the best way to get things done. This harshly antidemocratic orientation, Baumgart suggested, has now passed on through academic tradition to the next generation of liberal historians, who found new reasons to incorporate it into their own work. Thus, the picture of the cautious and antidemocratic liberal leaders of 1848 is really a historical myth, perpetuated and elaborated by certain unfortunate late nineteenth-century political influences. It has had mischievous consequences, because it has expunged from German historical consciousness the recollection of the truly liberal democratic revolution of 1848—the "repressed revolution" of the book's title.¹⁵

acceptable; see, for example, Paul Wentzcke, *1848: Die unvollendete deutsche Revolution* (Munich, 1938); and Heinrich Ritter von Srbik, *Deutsche Einheit*, 4 vols. (Munich, 1935–42). But in the late 1940s and 1950s, especially around the centennial year 1948, the liberal tradition was revived in its essential points. Influential examples include Wilhelm Mommsen, *Grösse und Versagen des deutschen Bürgertums: Ein Beitrag zur Geschichte der Jahre 1848–1849* (Stuttgart, 1949); and Friedrich Sell, *Die Tragödie des deutschen Liberalismus* (Stuttgart, 1953). Sell's book is extreme in its vehement denunciation of the radicals, and Frank Eyck's *The Frankfurt Parliament, 1848–1849* (Princeton, 1968) has a similar orientation. The German translation of Eyck's book, *Deutschlands grosse Hoffnung: Die Frankfurter Nationalversammlung, 1848/49* (Munich, 1973), contains an introduction by Carlo Schmid that spells out the present-day political implications; Schmid, Introduction to Eyck, *Deutschlands grosse Hoffnung*, 9–11.

¹⁴ Baumgart, *Die verdrängte Revolution*, 39–45.

¹⁵ *Ibid.*, esp. chap. 6.

This notion of a democratically oriented German liberalism that was forgotten after 1848 has been developed in some detail in a series of works on the parliamentary practice of 1848. It used to be said that the Frankfurt Parliament was a mere *Honoratiorenparlament*, an assembly of notables with little parliamentary experience, who gave good speeches but were regrettably deficient in the necessary tactical or organizational skills that make a modern parliament function. The revolution occurred in a basically prepolitical environment, without organized political parties that might have supported a bid for parliamentary supremacy and thus have secured the revolution.¹⁶ But in 1963 Gilbert Ziebura produced evidence to the contrary—evidence that the Frankfurt parliamentarians actually did organize disciplined parliamentary parties that effectively regulated both speaking and voting in the plenum. Furthermore, Ziebura showed that the members skillfully exploited constituency and interest-group pressures and that the parliament controlled the German Provisional Government by holding its ministry politically responsible to the assembly.¹⁷ The Frankfurt Parliament, he asserted, was no “ivory tower” but, rather, “a modern parliament of the French type.”¹⁸

A few years later, Helmut Kramer produced a well-researched monograph that substantiates Ziebura’s thesis in greater detail. Kramer tried to explain how these startling new developments in parliamentary practice could have occurred so suddenly. He admitted that very little in the pre-March German experience had prepared the way. Although some German states did have parliaments, their powers were limited, they had little influence on state executives, and their political organization was rudimentary. The men who led the opposition movements in these parliaments, moreover, were strong individualists who deprecated the sort of control that an organized party might exercise over its members. But Kramer maintained that in 1848 the pre-March parliamentary experience was quickly seen as irrelevant to the new situation. The men sent to Frankfurt had to create their own executive authority in the form of a German Provisional Government. The Provisional Government quickly became responsible to the Frankfurt Parliament because it had no other source of support. Kramer asserted that there are “unwritten rules of a system of parliamentary government” to which even this parliament of individualists was subject.¹⁹ These unwritten rules require strict party discipline and the cultivation of popular opinion; only through these methods can an incumbent government be assured of the reliable majority it needs to sustain itself. The built-in requirements of parliamentary government, Kramer therefore argued, led the Frankfurt Parliament to develop such surprisingly sophisticated and modern techniques of parliamentary organization.

Manfred Botzenhart has made the latest and most impressive contribution to this process of re-evaluating the moderates of 1848. His book deals not only with the

¹⁶ See, for example, Bruno Gebhard *et al.*, *Handbuch der deutschen Geschichte*, 3 (9th edn., Stuttgart, 1970): 145. This section of the book has remained unchanged through many editions.

¹⁷ Ziebura, “Anfänge des deutschen Parlamentarismus: Geschäftsverfahren und Entscheidungsprozess in der ersten deutschen Nationalversammlung, 1848/49,” in Gerhard A. Ritter and Ziebura, eds., *Faktoren der politischen Entscheidung* (Berlin, 1963), 185–236.

¹⁸ Ziebura, “Anfänge des deutschen Parlamentarismus,” 209, 203.

¹⁹ Kramer, *Faktionsbindungen in den deutschen Volksvertretungen, 1819–1849* (Berlin, 1968), 26.

Frankfurt Parliament but also with the parliaments of all the German states during these years. It is a comprehensive survey but has a thesis: not only were the moderate liberals of 1848 much more democratic than is usually supposed, but also, contrary to popular belief, they did seize power in 1848. Botzenhart conceded that they had not intended to take power. But, once the revolution was under way, they found themselves with a majority in every parliament in Germany and held membership in, if they did not actually control, most of the governments. They rose to the occasion, organized themselves in the fashion Kramer described, and established in many places functioning parliamentary governments. Most important, said Botzenhart, the moderates did not just gracefully accept a situation they had not made but became true converts to this new system of government. They even passed laws to anchor their techniques of parliamentary control firmly in the various new constitutions. Most important among these was ensuring the political responsibility of ministers to parliamentary majorities (not to the monarchs, as was traditional in the German states). This very advanced notion found its way into the constitutional documents of Hanover, Bavaria, and several smaller states and very nearly—it was excluded for purely technical reasons—into the Frankfurt constitution. The moderates, Botzenhart concluded, were determined to preserve an authentic system of parliamentary government in the new liberal regimes that they were creating for the future.²⁰

Botzenhart admitted that these liberals were not exactly democrats. They wanted to combine parliamentary government with a restricted franchise to ensure that they and their kind would continue to control these governments, but the radical minority in the Frankfurt Parliament forced the passage of universal suffrage. He also conceded that the moderates soon began to reconsider their commitment to the parliamentary system when they perceived that the democratic movement and the revolution were getting out of hand. In other words, this was a passing phase and not a permanent conversion on their part. Later German liberalism was not so careless with the prerogatives of monarchs, not so anxious to secure decisive popular influence over government. But Botzenhart did argue that the liberals were much more accommodating to democratic values in 1848 than they later became and that the system they worked out at that time might well have formed the basis for a genuine parliamentary democracy in nineteenth-century Germany. "It is one of the great missed opportunities of German history," he concluded, "that this chance was lost."²¹

It remains true that these early liberals, temporarily sympathetic though they may have been to democratic goals, put a great distance between themselves and the revolution: they had not wanted it, and they were very much afraid of it. How, then, can they be regarded as the true ancestors of modern liberal democracy, which to flourish has required such a revolutionary transformation of the old regime? Michael Neumüller addressed this problem in a study of nineteenth-century liberal attitudes toward revolution. By ingeniously distinguishing between

²⁰ Botzenhart, *Deutscher Parlamentarismus in der Revolutionszeit, 1848–1850* (Düsseldorf, 1977).

²¹ Botzenhart, "Die Parlamentarismusmodelle der deutschen Parteien, 1848/49," in Gerhard A. Ritter, ed., *Gesellschaft, Parlament, und Regierung: Zur Geschichte des Parlamentarismus in Deutschland* (Düsseldorf, 1974), 143.

means and ends, he could argue that the liberals of 1848 were true revolutionaries because their goals were revolutionary: they aimed at a thorough reorganization of society—its entire reconstitution on the basis of liberty. But they did not believe that this particular kind of reorganization could be effectively achieved by violence. Instead, they hoped that the state could be induced to make the necessary changes peacefully once its agents were persuaded that change was in society's best interest. Changes introduced by the state, however profound they may be, are called reform, and that is the sort of revolution the moderate liberals wanted: a "reform-revolution." Their goals were no less revolutionary because they sought official sponsorship, argued Neumüller. In this way he endowed the German liberal movement with an authentic revolutionary tradition, one that implicitly unites it with our own revolutionary age.²²

The liberal historiographic interpretation of 1848 remains dominant. Its hallmarks are admiration for the moderate liberals and assertion of continuity from them to the leaders of the present. The clear implication is that the Federal Republic of Germany is the heir to their aspirations. But in the late 1960s this historiographic consensus was upset by a number of scholarly works with a different implication. These new works deal with many aspects of 1848 and, even taken as a whole, do not present a consistently formulated alternative view. What they have in common is an admiration for the democrats of 1848, the moderates' left-wing rivals. And this difference in orientation makes it possible to identify them as a latter-day revival of the fourth traditional interpretation, the radical one.

The traditional radical interpretation has not had a prosperous career. It was born in the furious aftermath of the revolution itself, created by enthusiastic democrats who blamed their failure on the liberal betrayal. But this view quickly became moribund in the reactionary 1850s and remained so until the Weimar Republic. At that time a few historians broke ranks with their colleagues and tried to revive it, partly to provide the new regime with a respectable pedigree. The best-known of these efforts is the large two-volume history of the revolution by Veit Valentin, who tried to convince his fellow Germans that the revolutionary experience was an event of blessed memory of which they could rightly be proud.²³ But the attempt did not succeed, and radical revisionism disappeared after 1933, along with the republic itself.

After the war came the founding of the Federal Republic, which was, basically, Weimar without communists. The political climate of the 1950s did not encourage a resurrection of radical historiography, but the climate of the 1970s was very different. It produced a flood of historical revision, of which the literature discussed here forms a part. The political implication of this revived radical interpretation of 1848 is clear: the cautious and stodgy old Federal Republic needed to adopt a more democratic attitude. Why, the new revisionists seem to have been asking, should they continue to honor a political tradition that is identified with resistance to democracy? The moderates of 1848 were antidemocratic, as were the liberal historians of the empire and Weimar. West Germany is now a democratic state, and

²² Neumüller, *Liberalismus und Revolution: Das Problem der Revolution in der deutschen liberalen Geschichtsschreibung des 19. Jahrhunderts* (Düsseldorf, 1973), 183–247.

²³ Veit Valentin, *Geschichte der deutschen Revolution*, 2 vols. (Berlin, 1930–31).

its historical democrats should not be made to look sinister. Instead, historians should be showing that the democrats, the men of the left, had a credible program that the citizen of the 1980s can admire. Germany does have genuine democratic traditions, and the historians' task is to illuminate them in order to strengthen contemporary democracy.

Present-day political circumstances, however, do not encourage indiscriminate rehabilitation. There were several kinds of democrats in 1848, including communists and impulsive putschists like Friedrich Hecker and Gustav von Struve who recklessly overestimated their chances of success and probably did more harm than good. The authentic democratic idea, it is asserted, was not represented by these extremists but by those often called "liberal democrats"—in one case labeled "visionaries of the *bürgerlich-demokratische* social state," in another "liberal socialists."²⁴ They are now seen as responsible and clear-sighted men, distant precursors of the post-Godesberg Social Democratic party (SPD), men whose goals were what the Federal Republic stands for today—or ought to. To be sure, they were the "losers of history," for they did not prevail in their time.²⁵ But historians have paid entirely too much attention to the "winners" of history, like Bismarck, whose influence has not always been for the best. It is time, the revisionists have suggested, to pay attention to a more attractive precedent—that of the democratic left of 1848—and thereby to "reawaken" this tradition for the present.²⁶

Some of this work, such as Walter Grab's biography of Wilhelm Schulz, takes the form of democratic hagiography. The name Wilhelm Schulz is hardly a household word today, Grab noted, but it should be. Schulz was a member of the Frankfurt left, an early fighter for parliamentary democracy and social justice. He was a brilliant fellow: he even gave Marx some of his better ideas on economics. But, contrary to the prevailing stereotype of the the Frankfurt democrats, he was no wild-eyed revolutionary. Indeed, he specifically repudiated armed insurrectionists like Hecker and Struve. Rather, he believed that his ideal—a democratic federal republic that would introduce important social reforms to prevent future revolutions—could and would be achieved without the use of force. His belief in the parliamentary process was so complete that, when he found himself in the minority at Frankfurt, he submitted to the will of the majority and put his faith in the future. The obvious parallels between Schulz's ideas and the present-day constitutional order make it clear where the roots of the Federal Republic are planted.²⁷

This picture of a relatively tame and responsible democratic left has been given a

²⁴ "Left liberal" was employed by Bernhard Mann, "liberal socialist" occurs in Sander L. Gilman's collection of Blum's works, and "visionaries" is the term used by Walter Grab; Mann, *Heilbronner Berichte aus der deutschen Nationalversammlung, 1848/49* (Heilbronn, 1974), 1; Gilman, ed., *Robert Blum: Politische Schriften*, 1 (Nendeln, Lichtenstein, 1979): i; and Grab, *Ein Mann der Marx Ideen Gab: Wilhelm Schulz, Weggefährte Georg Büchners, Demokrat der Paulskirche—Eine politische Biographie* (Düsseldorf, 1979), 11. Dieter Langewiesche identified them as "compromising" republicans, as opposed to the "uncompromising" republicans of the extreme left; Langewiesche, "Republik, Konstitutionelle Monarchie, und soziale Frage: Grundprobleme der deutschen Revolution von 1848/49," *Historische Zeitschrift*, 230 (1980): 529–48.

²⁵ On the "losers of history," see Grab, *Ein Mann der Marx Ideen Gab*, v; and Jörn Brederlow, "Lichtfreunde" und "Freie Gemeinden": Religiöser Protest und Freiheitsbewegung im Vormärz und in der Revolution von 1848/49 (Munich, 1976), 7.

²⁶ Grab, *Ein Mann der Marx Ideen Gab*, 9.

²⁷ *Ibid.* Also see Grab's Introduction to his edited volume, *Die Revolution von 1848/49: Eine Dokumentation* (Munich, 1980), 12–26.

more sophisticated treatment in two recent books that are devoted largely to political theory. One is Rainer Koch's political biography of Julius Froebel, a member of the extreme left in the Frankfurt Parliament. Koch has described Froebel as essentially a classical liberal individualist with an admixture of very early social democracy, similar to, but more sophisticated than, Grab's Schulz. Froebel's basic assumption was that the individual possessed inherent natural rights that the state was obliged to protect and guarantee. For that reason the state had to be limited, and the institutions of federalism and the separation of powers, including judicial review, were necessary to secure that limitation. In the long run, he believed, popular sovereignty through universal suffrage and representative government was the best political guarantee of liberty. Taking all this into account, Koch claimed that Froebel was "not the Robespierre of the revolution of 1848/49 but, rather, its Lafayette."²⁸ But Froebel also recognized that even these legal and political devices were insufficient. To be truly free the citizen needed access to education and the necessities of life. The state had to provide those things as well. So Froebel gave Germans an early vision of "social democracy," similar to but not the same as the present variety. Froebel was firmly anchored in a preindustrial world of economic individualism, and he never envisioned the specific problems of industrialism. That was generally true, said Koch, of the democracy of 1848.²⁹

The second work on political theory is Peter Wende's investigation of six men of the Frankfurt left—Froebel, Struve, Arnold Ruge, Karl Hagen, Karl Nauwerk, and Johann Wirth—who, he claimed, supplied a coherent theory of "democratic radicalism" for their parliamentary colleagues. Wende's democratic radicalism amounts to the sort of democratic liberalism that Koch found in Froebel, but Wende also emphasized the strong element of "left-Hegelian optimism" common to most of these thinkers.³⁰ This was expressed in their theory of history, which made the free movement of people the agent of progress: laws and constitutions must, of course, protect individuals, political minorities, and, above all, private property, but they must not be permitted to freeze the status quo; people must be free to make their own history within a democratic political order. The democratic order, they believed, should facilitate popular control over government through such means as frequent elections and recall of elected officials. These men were committed above all to the protection of the rights of the individual. They were not naive Jacobins who would have led the country into a reign of terror but, essentially, liberal democrats who wanted to free the citizen from a tyrannical state.

These works illustrate the radical interpretation's present incarnation: the heroes of 1848 were members of the democratic left, who are now seen as responsible liberal democrats rather than as reckless revolutionary Jacobins.³¹ They formed the essential majority of the popular democratic movement, while irresponsible revolu-

²⁸ Koch, *Demokratie und Staat bei Julius Froebel, 1805–1893: Liberales Denken zwischen Naturrecht und Sozialdarwinismus* (Wiesbaden, 1978), 117.

²⁹ *Ibid.*, 1–2, 150, 157–58.

³⁰ Wende, *Radikalismus im Vormärz: Untersuchungen zur politischen Theorie der frühen deutschen Demokratie* (Wiesbaden, 1975), 104–05.

³¹ It is worth noting that the East Germans have also been paying more attention to the non-Marxian democratic politicians of 1848, acknowledging their good intentions but emphasizing their mistakes in judgment and attributing those mistakes to ideological errors. See Siegfried Schmidt, *Robert Blum: Vom Leipziger Liberalen zum Märtyrer der deutschen Demokratie* (Weimar, 1971), esp. 172–209; and Günther Hildebrandt,

tionaries formed a fringe group. This is the explicit thesis of Bernhard Mann's enthusiastic study of Württemberg in the revolution. Mann's hero is Friedrich von Römer, and Mann also rehabilitated Römer's colleagues and associates, including Friedrich Uhland, Paul Pfitzer, Gustav von Duvernoy, and Adolf Schoder. Römer was simultaneously a deputy at Frankfurt and the prime minister of Württemberg. At Frankfurt he worked for the abolition of the aristocracy and the destruction of the political power of the Württemberg nobility; the resulting democratization of Württemberg would have strengthened his position in Stuttgart and enabled him to support more effectively Frankfurt's efforts to unify Germany. As Württemberg's prime minister, Römer brought its government under parliamentary control. He took charge of institutions ordinarily under the royal prerogative and in some important confrontations forced the king to give in to parliamentary demands—for example, to recognize the German constitution promulgated at Frankfurt in April 1849.³²

This policy was extremely popular, according to Mann, for the Württembergers desired democracy, although they wanted it to be instituted by legal means—a legal revolution. Römer's policy reached its limits, however, when, after Prussia rejected the German constitution, the rowdy revolutionary left staged an armed uprising to force its implementation. Realizing that the resulting intervention by Prussian troops might lead to the eventual defeat of the whole revolution, Römer refused to support the insurrection. His strategy was to protect the legally secured democratic reforms in Württemberg and to argue for the legality of the German constitution but to give the Prussians no excuse for intervention by going even one step beyond the law. Mann strongly implied that, if the extremists had not revolted but had backed Römer's policy of strict legality, the German revolution might actually have reached its goals—that is, the force of public opinion might have prevailed over the lawless violence of the counterrevolution.³³ The effect of Mann's analysis is to vindicate the liberal democratic left by blaming the defeat of the revolution on its misguided rivals to the left and to the right: on the timid moderates who would not go far enough and on the violent revolutionaries who went so far that they precipitated the counterrevolution.

Other scholars have analyzed the popular movement within the context of the phenomenon of the *Vereine*, the voluntary associations that came into existence in huge numbers in 1848 as the old legal prohibitions against free association suddenly disappeared. Actually, voluntary associations had been around for a long time, at least since the late eighteenth century. They were one of the means by which the German *Bürger* had tried to break out of the rigid social mold of the old regime and were important vehicles in bringing about a modern civic society to replace the old-fashioned corporate society.³⁴ But these older associations had

Parlamentsopposition auf Linkskurs: Die kleinbürgerlich-demokratische Fraktion Donnersberg in der Frankfurter Nationalversammlung, 1848/49 (East Berlin, 1975), esp. 236–39.

³² Mann, *Die Württemberger und die deutsche Nationalversammlung, 1848–1849* (Düsseldorf, 1975), 318.

³³ *Ibid.*, 351.

³⁴ See Thomas Nipperdey, "Verein als soziale Struktur im späten 18. und frühen 19. Jahrhundert," *Geschichtswissenschaft und Vereinswesen im 19. Jahrhundert: Beiträge zur Geschichte historischer Forschung in Deutschland* (Göttingen, 1972), 1–44; Otto Dann, "Die Anfänge politischer Vereinsbildung in Deutschland," in Ulrich Engelhardt et al., *Soziale Bewegung und politischer Verfassung* (Stuttgart, 1976), 197–232; and Theodor Schieder

been, at least ostensibly, social and cultural in nature; those of 1848 were explicitly political. In 1967 Werner Boldt, in a trailblazing dissertation on the Württemberg *Volksvereine* of 1848, depicted them as grass-roots associations created by the democrats, first to get their candidates elected to parliament and then to support their representatives' parliamentary work. The *Volksvereine* also provided an open forum for political discussion, a means of education for a public unused to participatory politics. Eventually, they linked up with the Central March Association, a nationwide federation of *Vereine* created by the Frankfurt parliamentary left to gain an organizational arm within the electorate. Boldt saw the combined democratic parliamentary caucus and popular *Vereine* as a true political party in the modern sense or, more accurately, a "political body *sui generis* that went beyond simple beginnings to achieve an independent existence."³⁵

Subsequent scholarship has accepted Boldt's identification of the combined democratic *Vereine* and parliamentary caucus as a political party. For Dieter Langewiesche, a party is simply a group with political goals and the intention of electing members of parliament. By that definition, he has argued, the Central March Association qualifies as a legitimate national political party, the first in Germany. Furthermore, he asserted, its influence did not end with its dismantling after the revolution, for direct continuities of ideas and personnel connected the *Vereine* with the revived political parties of the 1860s and 1870s. In other words, the history of German political parties begins not with the founding of the Progressive party in the 1860s, as is commonly asserted, but with the German revolution of 1848.³⁶

Joachim Paschen has studied popular associations in Prussia, and he also has come to the conclusion that the "revolution of 1848/49 can be characterized as the birthday of the German political party."³⁷ But his claims go further than those of Langewiesche. Not only were the Prussian democratic *Vereine* numerous, well organized, and closely linked to the parliamentary groups at both Frankfurt and Berlin, but their purpose, Paschen asserted, went beyond winning elections to securing control over the state through the *Vereine*. This is party government, precisely the essential feature of the modern democratic state in the twentieth century. In effect, then, 1848 was the birthday not merely of the political party but of the modern system of party government as well. And the parent of these familiar modern phenomena is not just the revolution in general but the democratic left in particular.³⁸ Only the democrats had the political vision to foresee the ultimate

and Otto Dann, eds., *Nationale Bewegung und soziale Organisation: Vergleichende Studien zur nationalen Vereinsbewegung des 19. Jahrhunderts in Europa* (Munich, 1978).

³⁵ Boldt, *Die württembergischen Volksvereine von 1848 bis 1852* (Stuttgart, 1970), 2. In addition to this Heidelberg dissertation, also see Boldt's later work on the national parties of 1848, *Die Anfänge des deutschen Parteiwesens: Fraktionen, politische Vereine, und Parteien in der Revolution 1848* (Paderborn, 1971). This may be the place to note that the *Vereinswesen* of 1848 provides a field for social research quite apart from the political party organizations that have so far been studied. A good example of its potential is Stanley Zucker's "German Women and the Revolution of 1848: Kathinka Zitz-Halein and the Humania Association," *Central European History*, 13 (1980): 237–54, which is also one of the astonishingly rare entries in the field of German women's history for the early nineteenth century.

³⁶ Langewiesche, "Die Anfänge der deutschen Parteien: Partei, Fraktion, und Verein in der Revolution von 1848/49," *Geschichte und Gesellschaft*, 4 (1978): 324–61.

³⁷ Paschen, *Demokratische Vereine und preussischer Staat: Entwicklung und Unterdrückung der demokratischen Bewegung während der Revolution von 1848/49* (Munich, 1977), 9.

³⁸ *Ibid.*, 10–12, 158–61.

abolition of the distinction between ruler and ruled; only they could reach for sovereign power without ideological inhibitions. The pale efforts of the moderates to create an effective political organization foundered primarily on their inability to foresee these developments.³⁹ In that respect the Federal Republic—even modern democracy in general—can be seen as the heir of the democratic movement of 1848.

THE TWO PARALLEL INTERPRETATIONS OF THE REVOLUTION, one glorifying the moderates and the other glorifying the democrats, have all the elements of a controversy except confrontation. But neither lays exclusive claim to the truth, and, in fact, the confrontations have been few and relatively inconsequential. The most spectacular of them, initiated in 1970 by Federal President Gustav Heinemann, turned out to be a nonstarter.

Heinemann was an outspoken man of populist views who took an interest in the revolution, partly because his maternal grandfather had been a barricade fighter but largely because he felt that the Federal Republic needed a truly democratic history. In a much-publicized speech, he criticized the historical profession for not providing one. It was precisely the revolutionary tradition of the Forty-eighters, he said, that was relevant to the German present. To emphasize the point, he later opened at Rastatt—the place where the insurrectionaries of 1849 held out longest against the Prussian army—a memorial museum for German freedom movements. The profession generally rejected Heinemann's intervention, treating it as the outburst of an ill-informed outsider. In a rebuttal printed in the newspaper *Christ und Welt*, Theodor Schieder lectured the president, pointing out that "history is not a museum in which one can exchange at will the picture of this or that prototype."⁴⁰ Although a few of Schieder's colleagues adopted a more positive view of Heinemann's message, some even professing to be inspired by it, none has taken it literally.⁴¹ Historians have had little praise for revolutionaries as such.

Within the historical guild, attempts to bring about a confrontation have taken a different form. Two works have tried to solve the ambiguous legacy of the revolution by simply appropriating the whole thing for the democrats. One of these analyses, Werner Boldt's article in 1973, takes the position that only the democratic Forty-eighters have anything relevant to say to this democratic generation. The real question before the Frankfurt Parliament, Boldt argued, was whether to institute parliamentary government. Only the left really wanted it; the moderates fought hard to perpetuate a thinly disguised version of the old-fashioned *Obrigkeitsstaat*. The moderates were actually just enlightened conservatives who sought German unity for the economic and political advantages it would have brought but who were basically committed to maintaining absolutism in a slightly modified form.

³⁹ This is also a conclusion of Hartwig Gebhardt's *Revolution und liberale Bewegung: Die nationale Organisation der konstitutionellen Partei in Deutschland, 1848/49* (Bremen, 1974), esp. 156–62.

⁴⁰ Schieder, "Hat Heinemann Recht?" *Christ und Welt*, February 27, 1970, p. 11. For Heinemann's speech, see the description in Wollstein, "1848—Streit um das Erbe (I)," 492.

⁴¹ Grab, *Die Revolution von 1848*, 26; Boldt, *Die württembergischen Volksvereine*, v; and Klaus Goebel, "Politisierung und Industrialisierung," in Goebel and Manfred Wichelhaus, eds., *Aufstand der Bürger: Revolution 1849 im westdeutschen Industriezentrum* (Wuppertal, 1974), 225.

They had no serious ideas at all about individual liberty or democratic control. The moderates did practice a sort of parliamentary government at Frankfurt, Boldt conceded, but not because they were converted to the system in principle. They did not intend to continue the practice once a monarch was chosen to exercise executive power. Their aim was a German constitutional system of mixed powers that was designed to keep parliament in its traditional, subordinate position. All things considered, he said, "one ought to doubt if it is justifiable to apply the concept 'liberal' to them."⁴² To him, the democrats were the only true liberals in 1848.

Wolfram Siemann's published doctoral dissertation of 1976 arrives at the same conclusion. Siemann pointed out that a large majority of the Frankfurt deputies had been educated as jurists, most of them at institutions strongly influenced by the school of historical law. That school was profoundly conservative and specifically opposed to most of the ideas we call liberal: individualism, inalienable rights, equality of rights, popular sovereignty, and respect for the constitutional tradition stemming from the Enlightenment and the French Revolution. Siemann showed that the teachings of the school of historical law were constantly reiterated in the parliamentary speeches of moderates, and he concluded that the moderates were the standard-bearers of this antiliberal outlook in the Frankfurt Parliament. He argued, therefore, that to think of them as liberals is inappropriate. Rather, they were reformist monarchical conservatives.⁴³

Even this attempt to oust the moderate Forty-eighters from the liberal-democratic pantheon has not generated the controversy one might expect. It has neither made notable converts nor seriously ruffled the composure of those against whom it was directed. Each camp seems inclined to ignore the other. Mutual toleration is possible because, for all their differences, the liberal and radical interpretations of the revolution are not, in the larger context of all analyses of the revolution, so far apart. Radical historiography does make heroes out of democrats, but only the most moderate of them, and then precisely because of their moderation. And the protagonists of both views have been very conscious of their common rival, the socialist interpretation. The two views coincide on an array of sensitive matters that set them apart from the East German competition: for example, that the German workers of the time were reformist rather than revolutionary, that Marx had little influence on the working class, and that the revolution cannot be explained solely on the basis of class conflict.⁴⁴ Ultimately, these seem to be the more important issues.

One issue that vexed an earlier generation has faded away. No longer does either

⁴² Boldt, "Konstitutionelle Monarchie oder parlamentarische Demokratie: Die Auseinandersetzung um die deutsche Nationalversammlung in der Revolution von 1848," *Historische Zeitschrift*, 216 (1973): 620–21.

⁴³ Siemann, *Die Frankfurter Nationalversammlung 1848/49 zwischen demokratischem Liberalismus und konservativer Reform: Die Bedeutung der Juristendominanz in den Verfassungsverhandlungen des Paulskirchenparlaments* (Bern, 1976), esp. 285–86.

⁴⁴ The East Germans have tended to concentrate on Marx's influence in the Cologne *Arbeiterverein*; for examples, see Georg Becker, *Karl Marx und Friedrich Engels in Köln, 1848–1849: Zur Geschichte des Kölner Arbeitervereins* (East Berlin, 1963); and Joachim Strey and Gerhard Winkler, *Marx und Engels, 1848/49: Die Politik und Taktik der Neuen Rheinischen Zeitung während der bürgerlich-demokratischen Revolution in Deutschland* (East Berlin, 1972). The West Germans have emphasized the much larger *Arbeiterverbrüderung* and its leader, Stephan Born, who led the workers in a reformist and trade-unionist direction. For this approach, see Frolinde Balser, *Sozial-Demokratie, 1848/49–1863: Die erste deutsche Arbeiterorganisation "Allgemeine deutsche Arbeiterverbrüder-*

side assign exclusive blame to the other for the chauvinism of 1848, which staked out German claims in Schleswig, Posen, Bohemia, northern Italy, Alsace-Lorraine, and Limburg and would have brought Europe close to a general war that Germany would surely have lost. There seems to be general agreement that nationalistic short-sightedness was not the monopoly of any single political orientation, so neither the radical nor the liberal interpretations now make use of the issue.⁴⁵ The West Germans, in short, appear to have decided that they can live, though uncomfortably, with an ambiguous historical legacy so long as the ambiguities are contained within certain boundaries.

CONTRARY TO HAMEROW'S EXPECTATION of thirty years ago, the German revolution of 1848 remains a controversial subject. No consensus has yet materialized, and scholars' political persuasions often tend to guide their research. For the present generation, the ultimate consequences of the revolution are still matters of dispute. I do not mean, of course, that today's historians willfully twist the past for ulterior purposes. But even determined defenders of objectivity in the writing of history sometimes concede that honest and competent scholars can legitimately differ in matters of interpretation (provided their differences are controlled by empirical evidence) and that every historian's perception of the past is limited by a perspective, formed by personal experience and social and cultural environment,

ung" *nach der Revolution*, 2 vols. (Stuttgart, 1965). But the East Germans have argued that even the *Arbeiterverbrüderung* had its revolutionary moments, and they are publishing the organization's documents and reprinting its newspaper to substantiate the claim; Rolf Weber, ed., *Die Verbrüderung: Correspondenzblatt aller deutschen Arbeiter* (Leipzig, 1975); and Horst Schlechte, ed., *Die allgemeine deutsche Arbeiterverbrüderung, 1848–1850: Dokumente des Zentralkomitees für die deutsche Arbeiter in Leipzig* (Weimar, 1979). The recent reprint of Stephan Born's *Erinnerungen* (1898; reprint edn., Berlin, 1978) is perhaps a West German response. For a striking confirmation of the thesis of a revolutionary working class in one locality, see Eckhardt G. Franz, *Die hessischen Arbeitervereine im Rahmen der politischen Arbeiterbewegung der Jahren 1848–1850* (Darmstadt, 1975). Wolfgang Schieder conceded that there were both reformist and revolutionary workers in the labor movement of 1848; Schieder, "Die Rolle der deutschen Arbeiter in der Revolution von 1848/49," in Wolfgang Klötzer et al., *Ideen und Strukturen der deutschen Revolution 1848* (Frankfurt a/M., 1974), 56. For a sophisticated East German discussion of the relationship between class and political involvement, see Rolf Weber, "Die Beziehungen zwischen sozialer Struktur und politischer Ideologie des Kleinbürgertums in der Revolution von 1848/49," *Zeitschrift für Geschichtswissenschaft*, 8 (1968): 1186–93. The West Germans have usually found that the same social class often behaves differently in different places; for examples, see Christoph Klessman, "Zur Sozialgeschichte der Reichsverfassungskampagne von 1849," *Historische Zeitschrift*, 268 (1974): 283–337; and Hermann Josef Rupieper, "Die Sozialstruktur demokratischer Vereine im Königreich Sachsen, 1848–55," *Jahrbuch des Instituts für deutsche Geschichte*, 7 (Tel Aviv, 1978): 457–68.

⁴⁵ The threat of a fatal European intervention was perhaps first sharply formulated by Erich Marcks in "Die europäischen Mächte und die 48er Revolution," *Historische Zeitschrift* 142 (1930): 73–87; it was carried to an extreme by Alexander Scharff in *Die europäischen Grossmächte und die deutsche Revolution: Deutsche Einheit und europäische Ordnung, 1848–51* (Leipzig, 1942). Most recent studies have found the threat to be greatly exaggerated; see James G. Chastain's citation of works that show that England, France, and Russia did not seriously contemplate war with Germany in 1848; Chastain, Introduction to Rudolf Stadelmann, *Social and Political History of the German Revolution of 1848*, trans. James G. Chastain (Athens, Ohio, 1975), vii–xvi. The intentions of Schwarzenberg's Austria are much less certain, however, and that provides substance for recurrent reminders of the possible threat; for example, Frank Eyck, "Freiheit und Verantwortung: Chancen und Grenzen des parlamentarischen Systems," in Klötzer et al., *Ideen und Strukturen*, 100; and Michael Stürmer, "1848 in der deutschen Geschichte," in Hans-Ulrich Wehler, *Sozialgeschichte Heute* (Göttingen, 1974), 238. The wide dispersal of ultranationalism in the Paulskirche is shown in Günther Wollstein, *Das "Grossdeutschland" der Paulskirche: Nationale Ziele in der bürgerlichen Revolution 1848/49* (Düsseldorf, 1977).

that can probably never be completely overcome.⁴⁶ It is not surprising that 1848, Germany's sole authentic, modern revolution continues to generate controversy and disagreement; what is surprising is the narrow range of that controversy. The 135-year tendency toward ideologically compartmentalized historical writing is strangely sterile. A strong consciousness of the political implication of their work seems to have kept the German historians of 1848 in an interpretive rut and inhibited the development of a richer and broader historiography on the subject.

This situation may be about to change. In a survey of recent works on this general subject, Dieter Langewiesche rejoiced in their number, their quality, and even their tendency to alter or modify the isolated historiographical traditions of the *Erbschaftsverwalter* ("custodians of the various historiographical legacies").⁴⁷ His belief that the long-established historiographical pattern is finally changing may stem from his breadth of view. He cast his net widely enough to take in such topics as the general character of the liberal movement, the efforts of the German churches to cope with modernization, and several pioneering efforts to establish a German *histoire des mentalités*.⁴⁸ This broader perspective could blur the outlines of the inherited interpretations of the revolution; Langewiesche has perhaps been overly sanguine about the degree to which this has already occurred. He admitted that, in the end, "this confusing multiplicity" leads to "a confusingly differentiated picture of the revolution, one in which the *Erbschaftsverwalter* of all tendencies can find something appropriate."⁴⁹

Yet, in broadening the scope of inquiry, historians will stretch and bend the old interpretations until eventually their shapes are changed and new forms emerge.⁵⁰

⁴⁶ For example, Oscar Handlin, *Truth in History* (Cambridge, Mass., 1979), esp. 405–10.

⁴⁷ See Dieter Langewiesche, "Die deutsche Revolution von 1848/49 und die vorrevolutionäre Gesellschaft: Forschungsstand und Forschungsperspektiven," *Archiv für Sozialgeschichte*, 21 (1981): 458–98. This is an extremely useful bibliographical article that succeeds in relating a very broad diversity of scholarship to its central theme.

⁴⁸ Langewiesche, "Die deutsche Revolution von 1848/49," 475, 477, 485–87, 493–94. On the liberal movement, see especially Lothar Gall, "Liberalismus und 'bürgerliche Gesellschaft': Zu Charakter und Entwicklung der liberalen Bewegung in Deutschland," *Historische Zeitschrift*, 200 (1975): 324–56. Works on the German churches include Martin Brecht *et al.*, *Pietismus und Neuzeit: Ein Jahrbuch zur Geschichte des neueren Protestantismus* (Göttingen, 1979); and Martin Scharfe, "Pietistische Moral im Industrialisierungsprozess," in Burkhard Gladigow, ed., *Religion und Moral* (Düsseldorf, 1976), 27–47. For examples of the new intellectual history, see Helmut Sedatis, *Liberalism und Handwerk in Südwestdeutschland: Wirtschafts- und Gesellschaftskonzeptionen des Liberalismus und die Krise des Handwerks im 19. Jahrhundert* (Stuttgart, 1979); Ruth Geiger, *Zeitschriften 1848 in Berlin: Die Zeitschrift als Medium bürgerlicher Öffentlichkeit und ihr erweiterter Funktionszusammenhang in den Berliner Revolutionsmonaten von 1848* (Berlin, 1980); Rainer Wirtz, "Die Begriffsverwirrung der Bauern im Odenwald 1848: Odenwälder 'Excesse' und die Sinsheimer 'republikanische Schilderhebung,'" in Detlev Puls, ed., *Wahrnehmungsformen und Protestverhalten* (Frankfurt a/M., 1979), 81–104; and Martin Scharfe, "... die Erwartung, dass 'nun alles frei sei': Politisch-rechtliche Vorstellungen und Erwartungen von Angehörigen der unteren Volksklassen Württembergs in den Jahren 1848 und 1849," in Konrad Köstlin and Kai Detlev Sievers, eds., *Das Recht der kleinen Leute: Beiträge zur rechtlichen Volkskunde* (Berlin, 1976), 179–94.

⁴⁹ Langewiesche, "Die deutsche Revolution von 1848/49," 498.

⁵⁰ Other entries into the field, such as those by Eberhard Sieber and Heinrich Best, are hard to classify. Sieber's study of the University of Tübingen yields a highly differentiated picture of the various social strata of the town, showing concretely some of the complexities involved in discussing abstractions like "bourgeoisie," "artisans," "workers," and "students"; Sieber, *Stadt und Universität Tübingen in der Revolution 1848–49* (Tübingen, 1975). Best's massive quantitative study of grass-roots petitions to Frankfurt reveals, among other things, that many of the petitions were generated by activist businessmen forming political coalitions with workers' organizations—an interesting contrast to the stereotype of a frightened middle class anxiously drawing away from the mutinous lower orders; Best, *Interessenpolitik und nationale Integration 1848/49* (Göttingen, 1979). See Langewiesche, "Die deutsche Revolution von 1848/49," 488–92.

This process is inevitable and, in this case, long overdue, for time and change perpetually confound our efforts to analyze the past by dividing it into discrete categories of historical interpretation, however useful or necessary these efforts may seem for our present comprehension of it.

Reviews of Books

GENERAL

WILLIAM MCKINLEY RUNYAN. *Life Histories and Psychobiography: Explorations in Theory and Method*. New York: Oxford University Press. 1982. Pp. xiii, 288. \$19.95.

William McKinley Runyan has taken up the task of examining a number of methodological and conceptual problems encountered in the development of "life histories," a particular subject matter defined as "the sequence of events and experiences in a life from birth until death" (p. 6). The main contributions of the work, as the author sees it, are the identification of "common methodological problems involved in the understanding of individual lives," and the elucidation of "a set of questions and puzzles about understanding individual lives which are of both theoretical and practical importance" (p. 12).

Runyan's book is directed to various audiences and is therefore divided into chapters and sections that can be read selectively, according to the particular interests of the reader, whether clinical, belletristic, or historical. The work is nevertheless unified by two recurrent themes, one being "the search for appropriately rigorous criteria and procedures for evaluating and improving our understanding of individual life histories" from the several standpoints noted above, and the second being "the interplay of rational and intellectual considerations with social and political factors in the process of gathering and critically evaluating explanatory accounts of lives" (p. 14).

Runyan's work is characterized throughout by careful, balanced, and comprehensive discussions of the problems of life history, for example, the problems of retrospective falsification or the problems raised by the possibility of multiple interpretations of a life, which may well rely on the same data to arrive at opposite conclusions. The work is also characterized by thoughtful discussions of the conflicts, particularly theoretical conflicts, extant in the

literature, and by the author's ability to suggest reasonable resolutions of these conflicts and useful strategies that facilitate the pursuit of this kind of biographical enterprise. Indeed, the work is so thorough and accessible on these several counts that it might serve not only as the text of choice for students but also for professionals interested in exploring the problem for their own purposes.

Runyan is fully aware of how little life history or biography may appeal to sociologists, demographers, and psychologists. But he seems less aware of how little such work may also appeal to many historians, some of whom might even argue that biography is not historical because no matter from what standpoint the problem is addressed there is no way to tell how or whether any individual is typical of a group—so that biography provides no basis for the kinds of generalizations historians are concerned to make. The author contends that "The in-depth understanding of particular individuals is also a legitimate objective of intellectual inquiry and one of the fundamental levels of analysis" (p. 13) important for the social sciences and history. One notices, however, the insufficiency of more general discussion of how such inquiry becomes important to these other disciplines.

Such criticism aside, it must be emphasized that Runyan's work is meticulously constructed and has such a degree of theoretical and methodological rigor that it is immediately established as superior to the other works presently available, including especially those in the field of psychobiography. With no particular ax to grind and no need to outshout the critics of psychohistory as a sign of intellectual chic (or of contrition), the book is devoid of the kind of clamorous and often ignorant claptrap the subject matter seems so routinely to elicit. On the contrary, Runyan demonstrates here the kind of quiet rigor, comprehensive knowledge, and professional good sense that we must all be grateful to see.

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YOSEF HAYIM YERUSHALMI. *Zakhor: Jewish History and Jewish Memory*. (Samuel and Althea Stroum Lectures in Jewish History.) Seattle: University of Washington Press. 1982. Pp. xvii, 144. \$17.50.

Zakhor is "remember" in Hebrew. It is an ancient biblical injunction, as important as any religious commandment, whose observance had a central role in keeping the Jewish people alive through their dispersion of two millennia. *Zakhor* is the title of a short but remarkable book that discusses the millennial tension between the age-old Jewish commandment—and tradition—of remembrance and the relatively new Jewish interest in history. It contains the four lectures Yosef Hayim Yerushalmi gave at the University of Washington, Seattle, as the 1980 "Stroum Lectures in Jewish Studies."

Although the Jews are a people with long memory—even their weekly day of rest, the Sabbath, was something they were commanded both to remember and to observe—and they have lived for many centuries among peoples of whose culture historiography was an integral part, it was only in the sixteenth century that they themselves began to write history. Modern Jewish historiography is thus a foreign, borrowed element in Jewish culture and, as Yerushalmi points out, its rise coincided with a "decay in Jewish memory." He is, it seems, hesitant whether or not to welcome this development. He is not sure whether he wants to go along with Yudka, the hero of the Hebrew writer Haim Hazaz's short story "The Sermon," who demands that the history of Jewish woe from the fall of Masada in the first century (not in the second as Yerushalmi writes) to the return to Zion in the nineteenth should simply be considered a nightmare that is best forgotten. But he is emphatic in holding that modern Jewish historiography can never substitute for Jewish memory, and closes his book with the thought that a time may well come in which "a new consciousness will prevail that will wonder why so many of us were immersed in history." That is, if I interpret him correctly, he feels that ultimately *Zakhor* is more essential for Jewry than historiography.

Meanwhile, however, Jewish historical research, only a little more than a century old in its modern form, continues as it must, and Yerushalmi, Salo Baron Professor of Jewish History at Columbia University, is certainly one of its most thoughtful and original practitioners. Having read and enjoyed his *Zakhor*, I for one want to register my wholehearted appreciation of those few scholars, including Yerushalmi, who labor on introducing new viewpoints into Jewish historiography, and as a result of whose work the age-old tension between Jewish memory and Jewish history has begun to subside.

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WILLIAM H. MCNEILL. *The Pursuit of Power: Technology, Armed Force, and Society since A.D. 1000*. Chicago: University of Chicago Press. 1982. Pp. x, 405. \$20.00.

At the end of a distinguished career, Sir Charles Oman observed that to be a civilian historian of military affairs was a lonely and unrewarding business. He wrote, in an essay called "A Plea for Military History," "Both the medieval monastic chroniclers and the modern liberal historiographers had often no closer notion of the meaning of war than that it involved various horrors and is attended by a lamentable loss of life. Both classes strove to disguise their personal ignorance or dislike of military matters by deprecating their importance and significance in history."

The passage of time has not altered things. It is a paradox of the nuclear age that historians as private individuals spend some part of their day, usually at the breakfast table, fretting about such things as the risks of war and the social cost of military expenditure but, during the rest of it, pay no scholarly attention to such things and scoff at those who do. This causes a kind of professional myopia. In his new book, William H. McNeill notes that most historians of the Industrial Revolution pay little attention to the war that was raging while it gathered force, and, if they notice it at all, argue that it hindered rather than promoted British industrial development or had no effect either way, conclusions that our experience of more recent wars would be enough, one would think, to make unlikely. The fact is, as McNeill points out, that "both the absolute volume and the mix of products that came from British factories and forges, 1793–1815, were profoundly affected by government expenditures for war purposes . . . and military demands . . . went far to shape the subsequent phases of the industrial revolution, allowing the improvement of steam engines and making such critical innovations as the iron railway and iron ships possible at a time and under conditions which simply would not have existed without the wartime impetus to iron production" (pp. 211–12).

It is too much to hope that *The Pursuit of Power*, which abounds in examples like this of the understatement of the influence of war in history, will effect a mass conversion of the profession, but it may cause all but the most inveterate detractors of military history to consider whether they might not be mistaken. It is a book of formidable scope and daring generalization; it is, in turn, outrageously speculative and brilliantly persuasive; and, in this reviewer's opinion, it is the most exciting study of the development and social significance of war in a generation.

McNeill is as interested as Hans Delbrück was, in

his *Geschichte der Kriegskunst*, in the reciprocal relationship between a society and the military system that it supports and, for purposes of analysis, he distinguishes between what he calls command societies, like the medieval Chinese empire, Rome, Persia, Aryan India, and the Ottoman empire, and market societies, like the national states of the West. The former had leadership elites who were jealous of their authority and suspicious of the kind of innovation that unfettered commercial activity encouraged. Their economies were controlled, and their military weapons, the production of which was a state monopoly, were kept at levels sufficient to deter barbarian attacks on their borders but inadequate to support foreign adventure. They avoided policies that might subvert the traditional social structure. Thus, the Chinese empire, at a time when it had the capacity to extend its power and trade to the west by rounding Africa, terminated its activities in the Indian Ocean and forbade the construction of ocean-going ships.

Market societies were more willing to tolerate the activities of merchants and the innovations that these brought in train, and this was notably reflected in their military institutions. Commercial activity produced the taxable wealth that supported standing armies and stimulated the private competition in arms production that encouraged interstate rivalry and, in time, expansion at the expense of the static societies. "The arms race that continues to strain world balances in our own time," McNeill writes, "descends directly from the intense interaction in matters military that European states and private entrepreneurs inaugurated during the 14th century" (p. 70). He traces this relationship from then until the late nineteenth century, a period marked by institutional and technical discoveries (the expansion of mining and of the availability of iron, the differentiation and articulation of tactical bodies, the invention of drill, the coming of interchangeability of parts, the effects of developments in steam transport and of railroads and new breech mechanisms, and the growth of staff systems and ordnance and logistical departments) and the military-industrial collaboration that became pronounced after the naval scare of 1884 and continued until 1914.

McNeill believes that "the sovereignty of the market in military as in other forms of management [was] . . . an eccentric departure from the human norm of command behavior . . . that dominated ancient times and has reasserted itself with remarkable power since the 1880's" (p. 116). His pages on the origins of the First World War, the managerial revolution effected by that conflict and its successor, and the arms race and quasi-command economies of the years since 1945 will probably attract as much criticism from the specialists as they do support from those who agree with his strictures on the

contemporary military-industrial complex. No one will easily remain unmoved by the vigor with which he argues that "technical questions got out of control on the eve of World War I in the sense that established ways of handling them no longer assured reasonably rational or practically satisfactory choices" (p. 298) and that this situation has continued and worsened to the point that military-industrial elites that have eliminated the former freedom of the market and imposed an increasing degree of conformity and obedience on modern societies by the defensive efforts that they demand of them are caught in a trap of their own devising, a frenzied race to pile up increasingly sophisticated weapons that cannot be used without blowing us all up.

McNeill is so pessimistic about the degree of political initiative left to the national actors in this competition that he calls in his last pages for the establishment of a global sovereign power that will exercise a monopoly of atomic weaponry. Despite his glowing description of the blessings that would flow from this, his proposal is as utopian as the dreams, which periodically affect those in high places, of wondrous new devices that will render all existing weapons useless. If there is a way out of our dilemma, it will probably be found by the tedious and patient expedients of diplomacy and the slow elaboration of arms control under the pressure of a public that is perhaps not as conformist as McNeill fears.

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EMMANUEL LE ROY LADURIE and JOSEPH GOY. *Tithe and Agrarian History from the Fourteenth to the Nineteenth Centuries: An Essay in Comparative History*. Translated by SUSAN BURKE. New York: Cambridge University Press or Éditions de la Maison des Sciences de l'Homme, Paris. 1982. Pp. ix, 206. \$34.50

Of all the forms of *histoire serielle* pursued by French and other European historians in the last two decades, the study of tithe records must rank as the least fruitful. Time series of vital events, prices, toll receipts, shipping volume, wages, and *vendange* dates are, none of them, devoid of important problems of source criticism and methodology. But none of these can compare, in sheer messiness, with the tithe.

On the face of it, the use of tithe records in agrarian history is sublimely straightforward: the church collected a fixed share, usually one-tenth, of the gross farm output. If one knows the size of that share, one knows the volume of gross output. But matters were almost never that simple. Not all crops were subject to tithe, the church often alienated the

collection rights, the recorded revenues are often those of leases rather than actual collections, farmers sometimes resisted payment, and collectors sometimes took the offensive to increase or restore their "take."

These manifold difficulties of interpretation have not dissuaded agrarian historians from using tithe records. The Seventh International Economic History Congress (Edinburgh, 1978) adopted the tithe as one of its themes, which provided an occasion for the dozens of local and regional studies to be gathered together. Many of those papers still await publication, but we now have a volume that reflects the state of research on the tithe and ventures to present a brief comparative history of European agricultural production. *Tithe and Agrarian History from the Fourteenth to the Nineteenth Centuries* consists of two essays. The first, by Joseph Goy, surveys methodological issues. It is, regrettably, a testament to the apparent intractability of the problems. Goy raises most of the relevant issues, but then becomes entangled in the disputes and assertions of other scholars, only to conclude with a plea to historians to embark on "an immense programme of work" in the collection of ever more tithe records.

The second and longer essay is Emmanuel Le Roy Ladurie's "Comparative Study of Trends." This is a guided tour of the history of grain production in France, with excursions to other countries, from the fourteenth through eighteenth centuries. Le Roy Ladurie is an engaging guide, and his account is liberally sprinkled with tidbits of information drawn from the specialized tithe literature, but experienced travelers to agrarian Europe will find his tour familiar. He presents the *grand cycle agricole* encased in the *histoire immobile*, in an account that is inspired by, but not really based on, the tithe studies. He is careful to point out the historical exceptions to his organizing framework, and he acknowledges the ambiguities that adhere to the interpretation of tithe studies. But in his view none of this is sufficient to erode the basic historical fact that Malthus and Ricardo reigned until the nineteenth century.

This book is essential for anyone interested in recent Continental research based on tithe records, but I do not believe it will convince anyone that tithe records provide a solid basis for generalizations about farm production in European agrarian history.

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PEREZ ZAGORIN, *Rebels and Rulers, 1500–1600*. Volume 1, *Society, States, and Early Modern Revolution: Agrarian and Urban Rebellions*; volume 2, *Provincial Rebellion: Revolutionary Civil Wars, 1560–1660*. New York: Cambridge University Press. 1982. Pp. viii,

280; vii, 231. Cloth the set \$75.00, paper the set \$19.90.

In order to gain full benefit from Perez Zagorin's *Rebels and Rulers*, a reader must first banish several misconceptions—misconceptions unfortunately promoted by Zagorin himself. The *first* is that the book provides a new, unified account of European revolutions and rebellions from 1500 to 1660. It does not. The two volumes present a well-informed, if often debatable, summary of existing knowledge concerning a number of rebellions in France, England, the Spanish empire, and Germany during the sixteenth and seventeenth centuries. The *second* misconception is that the two introductory chapters, which deal with conceptions, typologies, and explanations of revolution, form an integral part of the book. They do not. For those first fifty-five pages Zagorin adopts an irritable tone, flicking aside purported definitions, models, and explanations like ashes from his vest. At the end of the introduction, what remains is a delimitation of the book's topic as attempts by subordinate groups to change government, regime, or society by force; a rejection of the distinction between revolution and rebellion; and a clear identification of Marxists as the most dangerous of the many analysts to be avoided. Since Zagorin reintroduces definitions and criticisms as he goes, includes in his analysis a number of events (for example, the French urban rebellions he describes as "devoid of programs or ideas" [vol. 1, p. 244] that his definition excludes) and organizes most of the book around the distinction between rebellion and "revolutionary civil war," it will save time and confusion to start reading the book on page 61 where, with the presentation of the "society of orders," a tone of thoughtful explication succeeds the previous impatience. The *third* misconception is that the two volumes contain the "broad comparative synthesis" advertised in the jacket blurb. Although Zagorin does follow a number of other scholars in seeing the reaction to absolutism as the central feature of early modern rebellions, he denies them any element of class conflict and distinguishes implicitly between the principled opposition embodied in elite-led civil wars and the response to anger, hunger, misery, and despair characteristic of popular rebellions. He repeatedly shies away from opportunities for systematic comparison (as on page 235, where he attributes the absence of urban revolts in Tudor and Stuart England to "special historical circumstances") and generally employs comparisons to bring out the particularities of individual cases and to place them in their historical contexts. Zagorin's closing of the case studies and of the two volumes with a two-and-one-half-page "epilogue" reflecting on rebellion and revolution *since* the seventeenth century, instead of any conclusion, offers a

clear enough sign of his disinterest in comparative synthesis.

These misconceptions destroyed, a reader can benefit from *Rebels and Rulers*' clear summaries of a great body of scholarship in English, French, German, Dutch, and Spanish. After the superfluous introduction, the real first volume begins with a sketch on social and political organization in north-western Europe during the sixteenth and seventeenth centuries. There Zagorin takes positions on the standard issues: Was the English monarchy absolutist? (Yes.) Was the seventeenth century a time of crisis? (In a limited sense, especially in France.) Were the great Protestant rebellions distinctively revolutionary? (No, although they share some characteristics with twentieth-century mass movements.) In laying out the context, he gives remarkably little weight to the development of capitalism and the expansion of war. The dominant processes for Zagorin were the growth of strong monarchies, economic fluctuations, and reform.

From there, the first volume moves to compact presentations of agrarian rebellion (notably the German Peasants' War, Ketts' Rebellion, and French rural revolts) and urban rebellion (again French, and then those of the Spanish monarchy, including the great Comuneros Rebellion of 1520 in Castile and the 1647 Naples revolt). Given the prominence of popular attacks on enclosing or commons-abusing landlords in Ketts' Rebellion, and the frequency with which poor French people directed their indignation at local capitalists who had contracted to collect new royal taxes, Zagorin resists with remarkable determination any suggestion of class conflict in these events. He also underestimates the fundamental role of war, preparation for war, and payment for war in their genesis. Nevertheless, as descriptions his accounts hold up very well.

The same is true of the accounts in volume 2. There Zagorin sketches in fifty pages on province-wide rebellions (French Huguenots in the 1620s, Languedoc in 1632, Normandy in 1639, Spanish Moriscos from 1568 to 1670, Tudor rebellions including the Pilgrimage of Grace, and the important revolts of Aragon, Catalonia, Portugal, Scotland, and Ireland). The sketches serve as prologue for four case studies averaging forty or fifty pages: the French wars of religion, the revolt of the Netherlands, the English Revolution, and the Fronde. Having a researcher's knowledge of the Fronde alone, I found Zagorin's other three sketches useful, but wondered how he could describe French provinces as being in "anarchy" after 1648 (p. 211), ignore the critical position of parlements as links between national and local actors in the rebellion, overlook the continuation into the Fronde of the same well-articulated popular grievances against royal fiscal policy that had informed resistance to

the crown's agents over the previous decade, and deny Sol Westrich's substantial evidence of class conflict in Bordeaux. That reaction, however, makes the point. Zagorin's actual work of synthesis does not operate at the level of revolution in general, at the level of early modern rebellions and revolutions as a whole, or even at the level of a class of early modern rebellions. It operates on the individual event. *Rebels and Rulers* provides thoughtful summaries of many early conflicts, informed by a good sense of the political and social structures behind them, a hostility to forcing them into general schemes, and an aversion to interpreting them as expressions of class division.

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JOHN CHILDS. *Armies and Warfare in Europe, 1648–1789*. New York: Holmes and Meier. 1982. Pp. 216. \$27.50.

John Childs asserts in his preface that although it is "now generally accepted that Europe experienced a 'military revolution' between 1550 and 1650 resulting in the foundation of the national standing army, little attention has been given in English to the working out of that revolution during the second part of the seventeenth century and the whole of the eighteenth." To remedy this shortcoming (and the reviewer believes that much more has been written about the later than the earlier period and also feels that "standing," but not "national," armies had been founded), Childs has produced in this slim volume an "unconventional military history of Europe" from 1650 to about 1789. The book is addressed to the "general reader" and there is little in it that will be new to the professional military historian. There are, however, some observations on minor states and some statistics that would interest the specialist, but unfortunately the author has provided no documentation to buttress his data.

The book is comprised of five chapters, really separate essays. Two of the chapters, "The Limitation of War" and "The Other Side of the Hill," show the continued horrors that war brought to the civilian population. They also stress that the prodigious augmentation of armies created great burdens for society as well as problems for the governments. This theme also is pursued in the chapter "Barracks and Absolutism," discussing once again the interrelationship between permanent military establishments and absolute rule. The remaining two chapters on "The Armies" and "The Conduct of War" deal with military administration, organization, and operations and focus on the age of Frederick the Great of Prussia. Little, perhaps too little, is said

about operations in the period 1648 to 1700 (that is, before formations became totally linear), and the same is true about the debate on strategy and tactics during the last decades before the French Revolution.

Throughout the book the author has painted on a very broad canvas, choosing his examples from a wide range of states from England and Russia, Austria and Prussia, to Sweden, Poland, the Netherlands, and minor polities such as Julich-Berg. The treatment, however, is not systematic, and there are inconsistencies and important omissions. Nothing, for instance, is said about the Porte, still a major European military power; and neither Prince Eugene nor the Comte de Guibert, both pivotal figures, are mentioned at all. For that matter, chronological coverage also is uneven. Discussions of the limitation of war reaches back to the late feudal period, while tactics and strategy are dealt with in the framework of the mid-eighteenth century. Lacking a clear systematic focus, the great mass of data presented in this volume tends to confuse rather than enlighten the general reader.

There are some startling factual errors. What is one to make of the statement that the "Reichstag and the imperial states" were reluctant to "grant" the Austrian Habsburg a "private army" and that it "was not until 1679 that the Austro-Hungarian empire was able to boast its first standing corps of regular soldiers" (p. 30)? The imperial diet and states certainly had no power to "grant" an army, private or otherwise, the first regular standing units of the Habsburg were incorporated in 1648-49 (and some would argue as early as 1535 in Croatia), and the Austro-Hungarian empire did not come into being until the Compromise of 1867.

To sum up, despite its considerable erudition, wide scope, and even occasional brilliant insights coupled with excellent writing, this is an uneven book that falls short of its avowed purpose.

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DAVID HARVEY. *The Limits to Capital*. Chicago: University of Chicago Press. 1982. Pp. xviii, 478. \$30.00.

David Harvey is a professor of geography, whose interest in developing a Marxist approach to the theory of urbanization led him instead to write this treatise on Marxian economics. His volume is a lengthy explication of Marx's economic theories, but while it is thorough and detailed, his account presupposes little prior acquaintance with the subject and could be followed by a determined beginner.

Harvey quite properly begins with an analysis of basic Marxist concepts like the commodity, use

value, and exchange value. He then shows how on this basis Marx elaborated his theory of surplus value and of capitalist accumulation, and illuminates the role of class analysis in Marx's thinking. The bulk of the book then goes on to discuss more and more detailed aspects of Marx's system, including such topics as credit and the theory of rent, which are frequently ignored in secondary treatments of Marx. Harvey sees himself as following Marx in expanding an initial set of simple abstractions through consideration of them in different, increasingly concrete contexts, thus progressively enriching our conception of how the capitalist system operates.

Although Marx's ideas about value, class, and exploitation are fairly well understood, few general works on Marx's economics explore the range of topics Harvey does. For this he deserves to be commended. Harvey believes that such apparently recondite issues as Marx's ideas about fixed capital or his critique of Say's Law not only reflect the working out of Marx's more familiar economic notions, but also reveal much about the nature of capitalism. Whether the reader with only an average interest in Marx's economics will persevere through this rather dry text long enough to be persuaded of this is another matter. Harvey's account of Marx is reliable, if not very novel, and where there are gaps in Marx's handling of certain topics, Harvey plausibly reconstructs the position Marx probably held.

A weakness of the volume, however, is that Marx's ideas are not evaluated as carefully and critically as one would like. Harvey is familiar with an impressive range of literature on his subject in both French and English, as evidenced by his footnotes and bibliography, but he does not come to terms with many of the difficulties that this literature has posed for Marx. His treatment of the labor theory of value exemplifies this. His defense of it against those neo-Marxists who would jettison it altogether is quite weak, but weakness here undermines one's confidence in the rest of the edifice. As the book progresses, one increasingly wishes that Marx's ideas were matched against alternative interpretations of the economic phenomena in question. Harvey is true to the holism of Marx, but by operating entirely within Marxist confines, the system he presents seems hermetically sealed.

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AILEEN KELLY. *Mikhail Bakunin: A Study in the Psychology and Politics of Utopianism*. New York: Clarendon Press of Oxford University Press. 1982. Pp. 320. \$29.95.

Aileen Kelly, in this sharply critical study of the celebrated Russian anarchist, claims to provide a

fresh interpretation of Bakunin's intellectual development. In fact, she offers little that is really new. Using familiar printed sources and no archival materials, she cannot be said to have altered the picture of Bakunin presented in earlier hostile accounts or to have added significantly to our understanding of his life and ideas. Like previous critical writers, she sees Bakunin as a psychological misfit who took refuge in millenarian fantasy. His political ideas, she argues, stemmed from personal drives and needs, above all the "need to achieve self-realization as a real or integrated personality." In revolution, according to Kelly, Bakunin found a "salvatory process of self-affirmation whereby man regains his lost unity." His theories, she says, illustrate "the despotic implications of a cult of spontaneity and personal wholeness whereby introverted, divided and unfulfilled personalities seek (whether unconsciously or not) to utilize mass movements as vehicles for the realization of their frustrated aspirations" (pp. 4, 97, 112).

Such an analysis, however, misleads more than it enlightens. Bakunin, it is true, identified his own quest for liberation with that of humanity as a whole, and personal dissatisfactions doubtless contributed to his radicalism. But to reduce his motives to a desire for "wholeness," to see his revolutionary militancy as purely psychological in origin rather than as inspired, at least in part, by social injustice, is to ignore the historical context in which he thought and acted. The result is a portrait of Bakunin that verges on caricature.

Kelly is perhaps too much out of sympathy with her subject to render a balanced appraisal. Bakunin is hardly immune from criticism, but Kelly exaggerates the darker side of his character, hammering away at what she terms his "colossal egotism," his "insatiable thirst for authority," his "demagoguery," and mania for conspiracy. His last years, she baselessly maintains, were marked by "acute schizophrenia" (p. 193). For Marx, by contrast, Kelly shows unequivocal respect. Her account of the famous contest between the two revolutionary titans for mastery within the First International is grossly one-sided; Bakunin's criticisms of Marx, his warnings that a dictatorship of the proletariat would herald a new form of despotism, are dismissed as "simplistic shemata" containing "no real insight into the problem of liberty" (p. 219).

To write any book is hard labor, and no one would deny that considerable intellectual effort has gone into the making of this volume. But its thesis will not bear the weight of emphasis that Kelly places on it. It fails, above all, to account for the impact of Bakunin's personality on his contemporaries and the widespread influence of his ideas. His exuberance and generosity, the vigor and freshness of his writing, never come through. Yet surely, as

Edmund Wilson once noted, there was something in Bakunin, something great-visioned and noble, that enabled him to inspire such intense devotion and to excite such extravagant dreams.

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MICHAEL PODRO. *The Critical Historians of Art*. New Haven: Yale University Press. 1982. Pp. xxvi, 257. \$30.00.

While Leopold von Ranke, Theodor Mommsen, Wilhelm Dilthey, and other German historians of the nineteenth and early twentieth centuries were transforming the way most history was researched, written, and interpreted, a transformation of equal importance, and also under a German aegis, was occurring in the history of the fine arts. Michael Podro of the University of Essex has traced the origins and results of that transformation through the growth of "critical" art history from its philosophical roots in the late eighteenth century to its final flowering in the second decade of our own.

Podro begins his study with an examination of the basic question that brought modern art history into being: how can the art of the past be understood by the men of the present? This in turn resolved itself into the related themes of making past art accessible to contemporaries and of trying to understand it as an expression of the mind's freedom to rationally order its perceptions of the external world through the creative process. Critical art history attempted to accomplish these ends by setting up complete or partial systematic criteria for evaluating the artistic past. Starting with its Kantian and Hegelian origins, Podro examines this approach through critical appraisals of specific art historians and their work, ranging from Karl Friedrich von Rumohr to Erwin Panofsky.

What emerges from this serial account is the importance art historians placed on theory and the interpretive systems it tended to foster. Although there are some notable exceptions that include Rumohr and Aby Warburg, interpretive techniques that emphasized social, political, and other contextual influences on art were less favored than those based on idealist or other philosophical assumptions that sought absolute ways of understanding art that essentially transcended the merely historical. Under these circumstances, facts and the brilliant analyses they could support were often used primarily as the raw material for increasingly subtle and complex theories of interpretation.

Ironically, little is left today of these critical systems. Podro concludes that their efforts to establish absolutes failed and notes that their main heritage is

in the variety of approaches and insights they provide the ongoing effort to comprehend past art. Similarly, art as an expression of the mind's freedom has given way to a more diverse philosophical concept closer to Ludwig Wittgenstein than Immanuel Kant or Georg Friedrich Hegel.

Podro's book is solid and reasonably well written, considering the often abstract and abstruse nature of the material. It does a service as both an accessible reference work on art historical theory and as a work of specialized historiography. For the non-art historian, however, Podro's lack of references to the important interpretive developments among German historians in the period he covers will seem a rather narrow approach to a subject that has broader implications. In this respect, his book can only point by omission to the need for a parallel examination of the equally uneasy symbiosis between philosophy and fact that bedeviled the likes of Ranke and Friedrich Meinecke.

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EUGENE LUNN. *Marxism and Modernism: An Historical Study of Lukács, Brecht, Benjamin, and Adorno*. Berkeley and Los Angeles: University of California Press. 1982. Pp. vi, 330. \$29.95.

Despite their obvious contrasts and tensions, Marxism and the modernist movement in art and literature have exercised a deep mutual attraction. Both speak from and to the discomfort and isolation intellectuals have often experienced since the days of romanticism; both find transformative energies waiting beneath the surface of our powerful but often inhumane and disenchanted world. Eugene Lunn examines the relations between Marxism and modernism as they appear in the careers of four figures who were representative of diverse orientations and possibilities within each: Gyorgy Lukács, Bertolt Brecht, Walter Benjamin, and Theodor Adorno. Their debates and confrontations, both actual and implied, reveal many facets of the modernist movement, and of Marxism as it has developed in the twentieth century.

Lunn provides an excellent and valuable account of this important and compelling subject. His book is solidly informed, insightful, imaginative, and thought-provoking. Each of the four figures emerges in his separate individuality, clearly etched in contrast to the others. Their very different backgrounds, their attraction to contrasting currents within modernism, and their appropriation of separate elements in Marx's legacy all take their place in the overall configuration of cultural modernism and political radicalism each sought to establish. Matters like Brecht's combination of cubist and constructivist

aesthetics with populist materialism, or Benjamin's alloy of Jewish messianism and surrealist antisubjectivism, are complicated and subtle. Lunn gives them coherent and intelligible shape, relating each one to the particular form of politics it subsumed or supported. The book is organized around the pairs created by actual debates, Brecht-Lukács and Benjamin-Adorno. But by the end the lines of tension and connection between each one and all the others take shape.

Lunn would not have been able to succeed so well without a firm admiration for his subjects. Probably most of his readers will agree that all four figures deserve the high valuation he places on them. Nonetheless, some may find that the celebratory tone conflicts with the deep tensions and problems all his forms of Marxist modernism betray. Despite what Lunn continually praises as "rich variety" and "immense fruitfulness" of the debates, they never narrowed the distance between Brecht and Lukács's narrow pragmatism in cultural questions on the one hand, and Adorno and Benjamin's ethereal distance from the world of nonintellectuals on the other. These are Lunn's own characterizations. But he does not take up their challenge to ask whether the union of Marxism and modernism grew out of weakness as well as strength. Adorno's Marxism was highly idiosyncratic, yet he nowhere assumed the accents of orthodoxy more intently than in his—partly successful—attempts to dislodge Benjamin from the latter's positive evaluation of technical progress in modern art. Benjamin for his part was attracted to Marxism partially for its hard materialism, and in its name he gave vent to judgments whose crudeness sometimes clashed starkly with the innate subtlety of his mind. In loyalty to Marxism, both Lukács and Brecht gave up antiauthoritarian positions that had originally made them critical of party hierarchy and control, Brecht in self-conscious disciplining of his own subjectivity, Lukács in confessional rejection of his deeply rooted personal idealism. Marxism led both to abandon the elements in their consciousness that were critical of authoritarianism, convincing each to identify such critical distancing with his own isolation from reality, and thus to suppress it. The manner in which such resolutions came about is the darker side of these personal histories. That side is perfectly visible in Lunn's admirable study; only his language sometimes veils it.

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DOUGLAS KELLOGG WOOD. *Men against Time: Nicolas Berdyayev, T. S. Eliot, Aldous Huxley, and C. G. Jung*. Lawrence: University Press of Kansas. 1982. Pp. x, 245. \$22.50.

This is an interesting, but in some respects curious, book. The subject is an important one, and in need of more study than it has received—though everyone knows in a general way that the twentieth century lost its predecessor's faith in progress. Time ceased to be a refreshing river and turned into a poisonous swamp. Tradition became not a source of creativity but an impediment, perhaps (Valéry) a wild beast clinging to man. Neo-orthodox theologians cut the tie between human social development and God's purposes, while artists and intellectuals sought, usually in vain, various rocks of stability against the buffetings of a mutability that now seemed irrational. It is a vast subject, related to declining standards of objective truth, to loss of social solidarity and stability, and humanity's declining control over its own destiny. Although the great 1914 war is frequently regarded as a landmark, many European intellectuals had already given up on reason and progress in the era of Nietzsche and Sorel.

Modernist art and literature, structuralist history and criticism, Einsteinian physics, analytical philosophy, and much else may be brought together around this theme. One suspects that Douglas Kellogg Wood began in an attempt to encircle it all but, in some desperation, finally decided to focus on a few key figures. They are interesting and relevant ones, but, despite his claim to the contrary (p. ix), they can hardly stand as complete microcosms of the revolt against time. A study of the time idea in this century that does not even mention (for a sampling) Einstein, Sartre, Wittgenstein, Lévi-Strauss, or the neo-Marxists can hardly be adequate to the subject. And concentration on them as individuals inevitably distracts the author into treating other aspects of his featured four—of whom, indeed, he provides attractive intellectual portraits. Although much of the information is not new to scholars, anyone wanting an introduction to these prominent thinkers can hardly do better.

I thought I found a few inaccuracies: Freud (p. 163) did not reject either the collective unconscious or occult phenomena, and the account of T. S. Eliot's breakdown accompanying the completion of "The Waste Land" (pp. 76–77), leaving out his marital difficulties, is hardly up to date. There are omissions, such as the strong influence on Eliot of Charles Maurras, significant surely for the time dimension with his desire to turn back the clock and freeze it in a premodernized Age of Reason and Absolutism. I thought the Huxley chapter the best, an incisive portrait of possibly the least weighty but certainly the most fascinating of this gallery.

The ways in which these writers tried to unbind themselves from the wheel of time are still (no pun intended) timely: Huxley's orientalist or drug-induced mysticism, Berdyaev's apocalyptic visions,

Jung's occultist mythology, the atavistic antirationalism of all of them are commonplaces of today's popular culture. Eliot appears out of place here perhaps, yet his search for a renewal of Christian consciousness is topical enough. Wood brings in some others, working Auden into the Eliot chapter along with Yeats, whom he refers to also in connection with Huxley but not, oddly, Jung, though James Olney in a work not cited by Wood pointed out startling similarities between the two. More might have been made of other such linkages. A book that appeared too recently for Wood to use it, Nancy Gish's *Time in the Poetry of T. S. Eliot*, suggests continuing interest in the subject. About Wood's engaging book we are left with the feeling of a vast subject not quite adequately handled, yet excitingly opened up, and most enjoyably written.

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GORDON A. CRAIG and ALEXANDER L. GEORGE. *Force and Statecraft: Diplomatic Problems of Our Time*. New York: Oxford University Press. 1983. Pp. xiv, 288. \$19.95.

Like eighteenth-century opera, the manual of diplomacy can represent one of the more decorous triumphs of the human spirit. The format can be that of a guide to practical conduct or a reflective history with theory implicit rather than labored. The best of these works counsel realism without cynical despair, predicate a casuistic (in the good sense) morality, and, avoiding the *Zukunftsmusik* of apocalypse or utopia for a suppler creed and style, accept the existence of competing sovereignties and of statesmen's ambitions as persistent shaping facts of global life.

Gordon A. Craig, who has eloquently called for the renewed study of international relations in his recent presidential address to the American Historical Association, has a fondness for the genre. Together with his Stanford colleague, Alexander L. George, who has written extensively on the sources of policy makers' behavior (from "operational codes" to the foibles of Woodrow Wilson) and on the framework of deterrence, he has produced a modern version of the diplomatic manual. *Force and Statecraft* is an ingenious combination of international-relations theory, historical case studies, and general lessons. The book has emerged as the fruit of a joint pedagogic effort, and some of the case-study chapters are based on their students' papers.

Sympathetic with "non-perfectionist" approaches to international politics, such as the "realist" counsels of Hans Morgenthau and Arnold Wolfers, the coauthors nonetheless recognize that moralistic cri-

tiques of power have a salutary role in setting standards for princes (pp. 269–78). Sympathetic likewise with detente, they carefully suggest how cautiously its negotiation must advance. In a cogent critique they evaluate the mixed results of Henry Kissinger's ambitious design, which failed to bring along American public opinion (pp. 132–34), even while they praise the step-by-step accomplishments of Willy Brandt (pp. 248–51).

The first portion of the Craig and George volume treats the history of the international system from the emergence of the great powers and contrasts the decentralized balance of the eighteenth century (analogous to the Smithian market); the post-Vienna "interpenetrated" alignment of Western and conservative courts; and the more rigid, bipolar balance of 1907–14. Along with a brief exposition of the successive concepts of Wilson and Roosevelt and the emergence of Cold War bipolarity, the authors review the impact of a democratized public sentiment with its pressure for staged and often insubstantial summits, and they touch on the troubling economic issues between the wars. The exposition is crisp and useful, although some historians might dissent on particulars: Is it valid to see Spanish decline only from the Thirty Years' War (pp. 5–6)? Did Bismarck's Reinsurance Treaty really overburden his alliance system (pp. 39–40)? Have not economic resources (including the mobilization of national revenue) been the sinews of force and statecraft all along and thus worthy of a more systematic treatment?

The second part of the book considers case studies of particular problems after an initial analysis of the negotiation process, including deterrence, coercive diplomacy (liable to escape control), and termination of wars. Occasionally the illustrative cases seem curiously chosen: deterrence was hardly the constitutive theme of the Congress system (save to restrain France in Latin America). Do the rather empty results of the 1973–75 Conference on Security and Cooperation in Europe justify its selection as the prime example of multilateral negotiation? Why not more focus on the pressing issue of arms races and disarmaments, including perhaps some review of successful negotiations (Washington, 1922) as well as failed efforts (Geneva, 1932–33, is treated only *en passant*).

All considered, historians of international relations must be grateful for this wise and useful handbook, which reinforces its predecessors' teaching that patience, moderation, and clarity about desired and feasible objectives remain critical gifts of statesmanship. Two major theoretical issues must persist. The first has to do with the "controlled case study": how usefully can it serve policy makers without denaturing the full depiction in the round that the historian demands? Must the reduction of

variables needed for the method sacrifice the imponderables that make lessons for statecraft so hard to derive?

The second issue has to do with the relationship of statecraft to systems. The Craig and George book elegantly presents both the maxims of statecraft and the historical evolution of international systems, even as it admits that the seismic pressures of the latter may overwhelm the capacity for wise statecraft. Can the diplomatic manual suffice in that case? Can its wisdom steer national societies through the possible catastrophes of systemic breakdown? The authors probably would agree that their counsels represent only a necessary minimum. They recommend forethought, sobriety, moral commitment as sheet anchor even for realists, and a knowledge of history. Weak defenses against catastrophe, but at least a beginning and too often more than we get.

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GEOFFREY BARRACLOUGH. *From Agadir to Armageddon: Anatomy of a Crisis*. New York: Holmes and Meier. 1982. Pp. viii, 196. \$22.50.

Geoffrey Barraclough, the very distinguished scholar of medieval Germany and, more recently, of other and more modern historical matters, has now turned to the second Moroccan crisis, of 1911. His small book is in the nature of an essay. Two essays, more precisely, one on the causes of twentieth-century wars and the other on Agadir as an example—and a particularly crucial one—of how and why twentieth-century wars get started. It is intended more as a contribution to thought and analysis than to historiographical erudition. The bibliographical essay at the end lists secondary works, and there are no footnotes or documentation in the text. There are aspects of this approach that may trouble professionals who read it: for one thing, on a number of occasions we are told what statesmen were thinking and feeling, and there are some generalizations that might bother the pros; on page 110, for example, the author observes of statesmen involved in the crisis that "as a group they were singularly neurotic and therefore frequently depressed." Such remarks suggest an intended audience wider and less finicky than one limited to fellow-scholars.

The first of the two more-or-less separable essays occupies the first six or so of the thirteen chapters and the last chapter. It is a discussion of ways and reasons why imperialism leads to war. There is a perfectly straightforward and familiar analysis of the colonial scramble, its motives and consequences, in neo-Leninist terms. It offers a good, if unsurprising, summary of the conditions of extreme unease

in which everyone lived in the years before 1914, of the particular anxieties of the European capitalist class, and of the unconscionable and sordid scheming of men like Joseph Caillaux. The last chapter, after the crisis itself has been described, continues this essay by pointing out that nothing much has changed in today's world since 1914, that capitalist societies are still conducting their foreign policies in ways and for ends hideously similar to those of 1911, and that the Reagan foreign policy is leading inevitably to another explosion that will, this time, certainly destroy the planet.

In between there is the account of the crisis. Close students may quarrel with some of its details; they may, in particular, feel that Barraclough considerably underestimates (and maybe misunderstands) both the motives and the effects of Churchill and Lloyd-George's defections from the dove group in the cabinet to the hawk group. They may also regret the slight breeziness with which statesmen's inmost thoughts are revealed. Still, it is an excellent account, and it has the merit of putting the emphasis on at least one of the places it belongs: the domino effect of Agadir that led immediately to the Italian attack on Libya and thence to the Balkan alliance and the quite ungovernable explosion of conflicting ambitions that in turn led to Sarajevo and the ultimatum.

There seems, however, to be a certain lack of consistency between the two essays. Little effort has been made to demonstrate that Agadir was in any significant way the product of the internal contradictions of industrial capitalism. Indeed, both the evidence and the author's statements would seem on many occasions to involve modification if not negation of that thesis. *Prestige*, he several times says, was the controlling concern in the making of both French and German policy. Kiderlen-Wächter was driven by the need to secure the support of the right in the Reichstag. Public relations of a sort designed to fortify political positions are indicated as a major consideration in most of the important decisions, and it is categorically demonstrated that neither the presence of Germany in the empty and arid coast of western Morocco nor the haggling over the wastes of the western French Congo were connected with any real economic or strategic considerations since, as he convincingly argues, none were involved. What was involved was party politics, public relations, prestige, and sometimes, as with both Kiderlen-Wächter and Caillaux, job security.

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ROBERT W. DESMOND, *Crisis and Conflict: World News Reporting between Two Wars, 1920-1940*. Iowa City: University of Iowa Press. 1982. Pp. xii, 518.

In the two decades that Robert W. Desmond covers in this, the third, volume of his history of international news reporting, the newspaper press reached the high-water mark of its development. At some point during these years, certainly in the West, there were more newspapers and more readers than at any time previously or since. Hence the questions of how the press dealt with international news and what influence this press exerted in the shaping of political cultures are of considerable interest. Apart, however, from inserting a few declamatory asides, Desmond has no intention of tackling such fundamental problems.

His credo is that of the classical liberal: "The importance of current information, accurately and promptly reported to all persons, can scarcely be exaggerated as basic to a proper functioning of the social order" (p. xi). In other words, if an open market of information and opinion is set up, the truth must win out. Given the author's assumptions, one might have expected, if not a book of analytical rigor, at least a reference work replete with useful and interesting information. The result is disappointing at best.

Desmond gives us three sets of impressionistic sketches: of the political history of individual countries, of the development of news agencies, and of the workings of major newspapers. For the first set the reader, it must be said, is better served by the appropriate section of William L. Langer's *Encyclopedia of World History*. Questions of balance and style aside, the blatant inaccuracies that abound in Desmond's *tour d'horizon* cannot be ignored. To refer to just one significant example, the Nazis did *not* have a majority in the Reichstag in July 1932 (p. 404).

The next two sections are, in the main, rosters of names, listing reporters and postings. Had the text been discarded, such a catalogue for the international press, assembled with care and precision, would certainly have been useful. Even here, however, Desmond's work does not inspire confidence. On a single page (p. 370), for instance, the following errors are to be noted: Lachmann-Mosse becomes Lackmann-Mosse; Benno Reifenberg is given the initial D.; the *Berliner Börsen-Courier* becomes the *Berliner Börsen Kurier*; and Arthur Koestler is listed as Moscow correspondent of the Ullstein papers in 1932-33, when in fact he was fired in May 1932 because of his Communist affiliations.

Many of these errors are no doubt hand-me-downs, but they are symptomatic of the failings of this book. As any historian who has worked with clipping files in newspaper archives will know, journalists, owing to the pressure of deadlines and their reliance on each other for sources, perpetuate each other's errors. Desmond, writing at the end of a lengthy career as a journalist and as a professor of journalism, should have tried to escape this trap. His

failure to do so is an ironic comment, not necessarily on the integrity of the ideological tenets he propounds, but on his practical relationship to his own credo. Indeed, the origin of this gap between ideal and reality in the Western press is perhaps the fundamental question to be asked about its development.

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RICHARD L. LAEL. *The Yamashita Precedent: War Crimes and Command Responsibility*. Wilmington, Del.: Scholarly Resources. 1982. Pp. xii, 165. \$19.95.

Richard L. Lael's study of the Yamashita case is the product of thorough research and well-constructed questions. His analysis and conclusions are generally persuasive, but not entirely so.

Lael begins with an excellent reconstruction of the Japanese plans for, and actual management of, the defense of Luzon in 1944 and 1945, the overall command of which had passed to General Tomoyuki Yamashita in early October of 1944, shortly before the American landings of Leyte and exactly three months before the major American landings on Luzon. Lael makes clear that the local Japanese naval commander, not Yamashita, was responsible for the devastating "defense" of Manila. He makes as good a case as any to date for Yamashita's lack of responsibility for the atrocities committed by men who were commanded by one of Yamashita's independently operating subordinates in the south of Luzon. Lael also offers good accounts of the preparation for the war crimes prosecutions, the crafting of the "should have known" principle of responsibility in the Yamashita trial, and the decision making and reasoning of majority and dissenting justices of the Supreme Court *In re Yamashita* and ensuing "command responsibility" trials (the Hostage case, the High Command case, and *U.S. v. Medina*).

One of Lael's conclusions is that General Douglas MacArthur did not "stack the deck" against Yamashita, as some have argued. Another is that the judges in the Hostage case offered the most balanced and reasonable delineation of the elements of guilt in command responsibility cases, somewhere between the broad sweep of Yamashita and the narrow, defense-oriented language of the High Command case and *Medina*. I find Lael convincing on both scores.

One of his central criticisms of the Yamashita court, however, is not as persuasive. Lael faults the court for failing to give adequate weight to the fact that the vast majority of the thirty-six thousand killings of Filipino civilians and U.S. and Filipino POWs "occurred during the month of February [1945], a time when Yamashita was over one hun-

dred miles to the north battling for his own units' survival" (p. 139). In the process, Lael explicitly discounts the 135 murders of civilians that occurred *within* Yamashita's immediate theater of operations, and he says nothing of the 400 murders that took place, *again* within Yamashita's immediate command, in early December 1944, a month before the American forces landed on Luzon and a week before Yamashita gave the order to divide his forces into three independent army groups. Together, these 535 murders cannot be said to have been committed by troops beyond Yamashita's ken, and 400 of them cannot be explained, as Lael suggests of many of the rest, in terms of combat stress (p. 132). Yamashita's trial, as Lael correctly points out, was not flawless, but Yamashita's guilt could still be said to have been established.

Moreover, given Lael's high regard for the standard set forth in the Hostage case, I find his relatively generous treatment of the military judge's instructions to the *Medina* court disheartening. Those instructions required "actual knowledge" of wrongdoing "plus a wrongful failure to act"; it was not enough to establish that the commander *should* have known of the misconduct of his subordinates (the Hostage case standard).

Consequently, I was puzzled by Lael's description of Articles 86 and 87 of the first protocol additional to the Geneva Convention of 1949 (agreed on by representatives of 150 nations in 1977). According to Lael these articles "finally vindicated" the military judge's instructions in *Medina* (p. 134) and "completed the erosion of the Yamashita precedent begun in 1948 at Nuremberg [the High Command case] and continued in 1971 [*Medina*]" (p. 135). In fact, Article 86 reads that commanders may be punished "if they knew, or had information which should have enabled them to conclude" that one of their subordinates "was committing or was going to commit" a breach of the law of war, "and if they did not take all feasible measures within their power to prevent or repress the breach." Article 87 reads that commanders must "prevent," "suppress," and "report to competent authorities" all violations of the law of war, and that they must "ensure that members of the armed forces under their command are aware of their obligations." Stronger language *might* have been chosen in 1977, but the language that *was* chosen would have been strong enough on which to base the charges against Yamashita.

As Lael points out, the State Department has yet to submit the protocols of 1977 to the Senate for consideration. Six years have passed. These are admirable protocols. Their adoption could help to reduce suffering in wartime. It is time to demand action.

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DAVID ARMSTRONG. *The Rise of the International Organisation: A Short History*. (The Making of the Twentieth Century.) New York: St. Martin's Press. 1982. Pp. x, 166. \$22.50.

International organizations are responses to the enlargement of contemporary political life. They are best studied historically, as products of their time. They are political creations, instruments of states, serving varied purposes. This brisk, reliable chronological survey of the League of Nations, the UN, the EEC, the main regional organizations outside Europe, and several contemporary "international regimes" never lets us forget that such institutions are only what their members let them be.

So David Armstrong's history is concerned essentially with the interests of states. There is only brief discussion of associationist thinking, international law, and institutional structure, just enough to show change as increasingly complex interaction brought forth new creations. The organizations are thus established squarely in the political matrix.

Armstrong does this well, and this is a strong addition to St. Martin's series "The Making of the Twentieth Century." The title's sense of a historical "rise" refers really to a shift away from security as the main purpose of these institutions to the less contentious goals of social welfare, economic development, and technical cooperation. The League might have been the triumph of hope over experience, but lessons were learned, sails trimmed, and we have now the proven functional validity of the many IGOs and INGOs of our day.

Armstrong is well disposed toward these organizations and what they represent. Some of his conclusions are generous. He does not prove that the advice of the permanent mandates commission was "generally taken seriously by the mandatory powers" (p. 41). Only a study of the archives will show to what extent it served to control arms and liquor traffic, repress slavery, and improve labor conditions. The commission was amazingly remote from its work. It used reports from the powers that dealt with events usually long past. No commissioner ever went, in an official capacity, to a mandated territory for an investigation.

Robert Cecil said that publicity was the lifeblood of the League. What did this mean, this open diplomacy? Cecil hoped it would be an antidote to the appeal of communism. Armstrong notes that the ILO was "originally envisaged as a safety valve against the spread of Bolshevism" (p. 43), but he does not discuss how this pertains. What were its accomplishments, if any? Today, the UN (and Third World associations) provide megaphones for otherwise barely noticed governments. This is a form of publicity, serving legitimization functions,

and is, of course, highly ideological. More needs to be said about these political attributes.

Armstrong gives a fine critical rundown on the OAS, the OAU, and ASEAN. He discusses the capacity of "international regimes" to establish procedures for dealing with human rights, terrorism, seabed exploitation, and, problematically, a "new international economic order." But it is still carefully preserved state autonomy and jealously guarded national power that make the world go around. Some recently acclaimed discussions of the danger of nuclear war ended in calls for world government. There is nothing in Armstrong's history to encourage that idea. International organizations of any significance continue to be the creatures of states. There are various kinds, but none studied here could, or was meant to, transcend the political milieu of which it was the product.

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ANCIENT

ERIK HORNING. *Conceptions of God in Ancient Egypt: The One and the Many*. Translated by JOHN BAINES. Ithaca: Cornell University Press. 1982. Pp. 296. \$25.00.

It is not often that an introduction to the thorny topic of ancient Egyptian religion can be recommended unreservedly, if recommended at all. We are so weighed down with the misapprehensions of over two millennia of willful ignorance or self-serving distraction on the part of our Greek and Roman forebears and Neoplatonic misfits closer in time to ourselves, that breaking free and viewing ancient Egypt fairly becomes surprisingly difficult. Over the past thirty-five years the acceptable introductions to Egyptian religion can be counted on the fingers of one hand, and all are from the pens of scholars trained in a Germanic tradition!

The present work by Erik Hornung maintains this excellent, though rare, standard. Published in 1971 under the (less apt) title, *Der Eine und die Vielen*, the book comprises a compact primer on the Egyptian concept of deity, rather than an introduction to cult and mythology (though these are mentioned in the course of the investigation). Hornung, sensing the absolute necessity of an inductive approach, begins with the requisite lexical study of the words used in Egyptian for "god." This leads to a useful and thoughtful discussion of one of the most difficult of concepts to grasp for us moderns, namely, the multiplicity of names and epithets as indicators of the essence of deity. What do we mean when we speak of "fertility" gods, or "war" gods and so forth?

What do we understand if we characterize a god as a "personification" or "Hypostasis" of something? To the Egyptians deity was basically a "power" somehow manifest in nature or society, and throughout history the parochial aspect of a god's responsibility—he was basically a township deity—kept him from being type-cast in a single role.

There follows an equally perspicacious investigation of how the gods manifested themselves in ancient Egypt. A common Egyptian notion was that the gods revealed themselves in a "myriad of forms," and that the universe was filled with them. Hornung rightly cautions against viewing this as pantheism, and follows with a treatment of the characteristics of the gods. The gods and mankind enjoyed a meaningful interaction during all periods that worked for the positive good of the optimistic Egyptians. Hornung points up the overriding importance to the Egyptians, in contrast to other ancient peoples, of the use of magic, which was found everywhere and in cult and everyday life, and which could even be used to frustrate the divine will. A chapter on the "Classification and Articulation of the Pantheon" concludes the study. The author has also included a useful bibliography and glossary of divinities (that might have been longer). There is an index.

John Baines's translation flows to the reader's ease, and as far as I could check, is reliable. I nonetheless lament a so-called "educational" system that does not equip youth or laity with the wherewithall to read such a book in the original!

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ROBERT DREWS. *Basileus: The Evidence for Kingship in Geometric Greece*. (Yale Classical Monographs, number 4.) New Haven: Yale University Press. 1983. Pp. ix, 141. \$18.50.

What's in a name? Not a rose but a *basileus* is the subject of Robert Drews's inquiry in *Basileus: The Evidence for Kingship in Geometric Greece*. Overall, he believes, to translate the Greek word as "king" is incorrect for the "semantic fields" of the two terms "are not at all identical" (p. 78). While the formalizing of *polis* institutions in the Archaic period created official positions for which *basileia* may be equated with kingship, for Greeks of an earlier period kingship did not exist: "The Dark Age polis surely had no king; it probably had no magistrates" (p. 130).

Yet the Dark Age did have *basileis*, or so traditional accounts would lead us to believe. Thus the core of the study is a careful examination of those traditions, a valuable compendium not only of the ancient evidence but of nineteenth- and twentieth-century views of that evidence. In interpreting the data, Drews argues that much of it is inaccurate; the

Argive, Athenian, and Corinthian king lists and accounts treating Geometric Ionia are particularly suspect. In cases where tradition is found to be more accurate, the polities are described as *ethnē*, not *poleis* (Achaia, Messenia, Arcadia, and, through the Dark Age, Laconia). Surely there is danger of a self-fulfilling argument here: where evidence for Dark Age *basileis* cannot be discounted, the polity must be an *ethnos*; in *poleis*, on the other hand, there is no *basileia* and hence the record must be faulty. Nor is the nature of the Dark Age record fully appreciated. In his rejection of the Homeric epics as reflections of their age, Drews misses the value of the oral tradition as the "orally stored experience . . . which incorporates the traditions of a culture group" (E. A. Havelock, *The Literate Revolution in Greece and Its Cultural Consequences*, [1982], p. 232). Moreover, his concern for precise definition does not extend to all Dark Age institutions and thus the reader finds *poleis* in existence in LH III (p. 111), their *politai* attacking a neighboring *polis* (p. 113). It is true that linguistic and institutional roots of the *polis* may be found in Bronze Age Greece but surely the nature of the Greek *polis* changed quite as much as did the Greek *basileia*.

And it is institutional change that should be the focus of a study examining *basileia*. Mycenaean Greece may have known officials termed, in Linear B, *pa2-si-re-u*; Dark Age *basileis* seem to have been leaders whose positions rested on personal characteristics; Archaic *basileis* were incorporated into formal political mechanisms of individual states. No single definition is suited to all periods. Further, since there was little uniformity between the hundreds of *poleis* and *ethnē*, so we should expect to find variety in the accounts as well as in the institutions of *basileia*. Drews's zeal to demonstrate that early Greek *basileis* were not kings has distracted him from a more fundamental task: definition of *basileus* as *basileus* within the changing context of early Greece.

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NANCY H. DEMAND. *Thebes in the Fifth Century: Heracles Resurgent*. (States and Cities of Ancient Greece.) Boston: Routledge and Kegan Paul. 1982. Pp. 196. \$19.95.

The introductory chapter of this book by Nancy H. Demand states the theme of recovery after the losses and disgrace of the Persian Wars. The figure of Heracles, appearing on Theban coins since the middle of the century, is noted as a symbol of laborious resurgence. A refutation of Athenian slanders ("Boeotian pig") is promised. The second chapter deals with the layout of the city, its economy, and its geographical position. Chapter 3, the longest, surveys "Theban political and military history dur-

ing the fifth century." The fourth chapter, "The cults of fifth-century Thebes," arranges figures of cult in the systematic relationships provided by myths. Chapters follow on "Philosophy in Thebes"; on "The Muses," (mainly a discussion of proficiency in flute playing and of Pindar); and on "The arts and crafts," (figured funeral *stelai*, and painted vases). The last chapter offers an overall picture of recovery after loss, with a turning point at 447, and devotes about half its space to the role of women. An appendix treats of "The position of women in Pythagoreanism." The bibliography is followed by two indexes.

"The focus of this book will thus extend beyond the usual material of political history to a broader consideration of the cultural and intellectual life of the city" (p. 4). Consequently many themes are treated and some are not treated in depth. For example, the pages on Pindar start from "the aristocratic code of values," digress into a comparison between homosexuality in Athens and in Thebes, discuss the importance of female choruses, and collect Pindaric passages on the afterlife; Elroy L. Bundy's work is not noted. Words like "oligarchy," "democracy," "moderate oligarchy," and "aristocratic oligarchy" are used as if their connotations were constant and known. Thebes is said to have been a "tribal state" as opposed to a "civic state," but as these terms are not explained, the effect is merely impressionistic.

The chapter on "Philosophy at Thebes" is the most successful. Demand scrutinizes two texts, Plato's *Phaedo* and Plutarch's *De genio Socratis*, with care and caution. Hence she asserts "a Theban Pythagoreanism which is basically mystico-religious in orientation" (p. 83).

The chapter on political history weighs the extant reports of successive incidents for credibility but does not make a systematic assessment of the respective value of Herodotus, Thucydides, and Diodorus on Theban affairs. Recounting the renewal of the Boeotian league in 447, Demand suggests a Pythagorean contribution; she does not ask the crucial question of power; how many of the eleven "parts" of Boeotia did Thebes control in 447–46? Questions about causes are not asked. The section on the Peloponnesian War begins with the sentence: "Thebes played an active and important role in this war from the very beginning" (p. 40). True, but why? Questions of geopolitics are not raised: how did the location of Thebes bear on the weight it exercised among the allies of Sparta? Demand notes that Thebes rejected the peace of Nicias but does not ask how Theban policy contributed to the ensuing increase of tension.

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N. G. L. HAMMOND. *Alexander the Great: King, Commander, and Statesman*. London: Chatto and Windus. 1980. Pp. x, 358. £14.95.

This new book adds another valuable study to the great number of biographical portraits of the Macedonian conqueror. Its author, N. G. L. Hammond, is a well-known classical scholar whose many books and articles have firmly established his reputation as a leading authority on ancient Greece and Macedonia. Like all the works Hammond has written during his long and prolific career, this work is one of highest scholarship. Hammond is well versed in the classical languages and has a masterly command over the ancient sources and a thorough familiarity with the voluminous modern literature on the subject. Moreover, he has a firsthand acquaintance with the topography of the Balkan countries and the Near East up to Western Iraq and the Libyan desert.

One of the principal merits of the book is the author's unsurpassed ability to reconstruct with precision Alexander's journeys and battles. Relying essentially on Arrian, and using his own experience in warfare and his knowledge of the terrain of the Balkans and the Near East, Hammond discusses at great length Alexander's Balkan and Asiatic campaigns and describes in minute detail the military tactics and strategy that reveal Alexander's military genius and superior leadership qualities. (Numerous carefully prepared maps, diagrams, battle plans, and other illustrations enhance the author's discussion.)

Although primarily a man of war whose extensive conquests gained for him the title of "the Great," Alexander's true claim to greatness, according to Hammond, lies in his vision as a statesman. For, instead of enslaving the native peoples of Asia or treating them as inferior barbarians (as Aristotle would have him do), he promoted a policy of fusion of European and Asian cultures and created "a supra-national community capable of living internally at peace and of developing the concord and partnership which are so sadly lacking in the modern world."

As in every study, there is, of course, some controversial material, and not everyone will agree with all of Hammond's views and conclusions. This reviewer, for example, disagrees with the author's contention (p. 250 and following) that Alexander as *hegemon* of the Greek league showed faultless conduct, never acting beyond his legal power. I rather agree with J. R. Hamilton's view that in many instances, particularly in issuing his Exiles Decree in 324 B.C., Alexander went far beyond the powers he possessed as *hegemon*. I am also somewhat hesitant to share the author's belief that the larger of the two royal tombs at Vergina (excavated in 1977 by Greek archaeologist M. Andronikos) is "almost certainly"

the tomb of Philip II of Macedon. The evidence, although tending to support this belief, is not conclusive; and some archaeologists are still reluctant to accept Andronikos's claim that he has found the tomb of Philip.

Such minor disagreements aside, Hammond's *Alexander the Great* is an important book. Based on a meticulous appraisal of the ancient sources, this carefully written and fully annotated study is perhaps the best-balanced and most comprehensive biography of Alexander that has appeared since the publication of Ulrich Wilcken's *Alexander der Grosse* in 1931.

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HERBERT GRASSL. *Sozialökonomische Vorstellungen in der kaiserzeitlichen griechischen Literatur (1.-3. Jh. n. Chr.)* (Historia, Einzelschriften, number 41.) Wiesbaden: Franz Steiner, 1982. Pp. vi, 231. DM 68.

In 1868 Gustav Hertzberg concluded that during the roman period educated Greeks had no interest in social and economic problems, and that view has been echoed ever since by scholars, including such luminaries as Michael Rostoutzeff, A. H. M. Jones, Moses Finley, and Pierre Vidal-Naquet. Now Herbert Grassl has argued exactly the opposite. He has collected an impressive mass of material, mostly from literary texts, which shows a continuing concern with contemporary problems. Grassl moves freely within the stated chronological limits, organizing his exposition around six major themes: municipal politics, balance of payments, population control, social problems, economic and fiscal problems, and class relations.

Several generalizations emerge from the analysis. Almost all Greek thinkers were in the grip of a cultural tradition—Grassl calls it “the Greek ideology”—inextricably linked with the city state; hence they had little to say on imperial problems (pp. 197–8). Closely connected, they were exclusively concerned with the thin stratum of the population in the cities, especially citizens. The arguments and conclusions they advanced had no effect on actual policy. For example, all condemned infanticide and exposure, but these practices continued unabated until the fourth century. It was Judeo-Christian moralists that ended them, not Greek intellectuals.

What this means is that under the empire culture and power were separated. Power was in the hands of imperial officials, while Greek thinkers concerned themselves mainly with the city. Or they retreated to an idealized golden age of equality and natural economy, best expressed in Plutarch's life of Lycurgus. Hence on imperial problems they had little to offer. The causes and significance of this were

indicated long ago by William Heitland in *The Roman Fate* (1922), a brilliant work that is—regrettably—absent from Grassl's extensive bibliography.

One important exception was Cassius Dio, a fact that emerges clearly from the able exposition of his views on political and fiscal policies (pp. 157–60). Dio was well informed, recognized the crucial issues, and was indeed remarkably prescient in his recommendations. He was, in short, quite aware that it was imperial problems that counted, and he was willing and able to deal with them very intelligently. Was this because he belonged to a class that had cast its lot with empire rather than cities? Or because he had had unusual experience in the exercise of imperial office? Or because of both factors (they were, after all, probably related)? Grassl rather brusquely dismisses the arguments of E. M. Schtaierman (pp. 6–7), and in general he has disappointingly little to say on the connections between class, concerns, and conclusions.

Otherwise the work is an admirable contribution to our knowledge of the period. The style is clear and mercifully terse, the copious footnotes give a comprehensive but not unselective guide to the literature, and an accurate index of sources facilitates reference. One thought does obtrude: Was the Latin tradition any different? As always, good answers lead to new questions.

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WAYNE A. MEEKS. *The First Urban Christians: The Social World of the Apostle Paul*. New Haven: Yale University Press, 1983. Pp. x, 299. \$19.95.

Wayne A. Meeks's object in this book is to recreate the social world of the Christian communities inspired by Paul in the middle of the first century A.D. He does so by filtering exegesis of the Pauline and deuter-Pauline letters through modern theories of group behavior, and by firmly setting the residue in the historical context of city life in the eastern Roman empire. The book is not as lucid and free of jargon as its dust-jacket maintains, but it provides a valuable demonstration of how the cautious application of sociological theory to problems in ancient history may yield new dimensions of understanding.

Pauline Christianity was an urban phenomenon, and Meeks begins by describing the largely uniform culture of the eastern cities, dwelling particularly on the common characteristics of those that housed the Pauline movement in Asia, Galatia, Macedonia, and Achaia. The treatment is balanced, though the purist will notice here errors of detail and over reliance on inadequate secondary sources. Next Meeks rejects the view that the Paulinists were

generically of very humble standing, and, justifiably arguing that to identify social status a network of factors must be considered, finds that the typical Paulinist was (like Paul) an artisan, but that community members as a whole—"independent women with moderate wealth, Jews with wealth in a pagan society, freedmen with skill and money but stigmatized by origin, and so on" (p. 191)—were marked by status inconsistency.

The communities that brought such people together were influenced by comparable, pre-existing forms (the private household, *collegia*, synagogue, and school); but their group boundaries were established through new, symbolic language patterns (of belonging, internally; of separateness, externally) and distinctive institutions (for example, common meals) that supplied internal cohesion. Group solidarity was advanced further through the new structures of leadership, which, although fluid, depended on the paternalism of Paul himself and the practical success as means of social control of such devices as the authoritative letter and personal visit. Ritual, especially baptism (an act of initiation not its prelude) and the eucharist, compounded cohesion and the communities' unity one with another; yet the Pauline cells were not rigidly exclusive, and in preserving links with the wider society from which they sprang, remained flexible and tension-ridden. In examining the social force of Paulinism, Meeks posits, finally, correlations between Christian monotheism and the unity of the Pauline groupings; between their members' conception of a personal God and the intimacy of their household congregations; between their eschatological change and social transformation; between the paradox of Jesus' death and exaltation and the groups' inner contradictions.

Paulinism appealed to social misfits. That is the basic proposition. It depends above all on the biographical register of thirty adherents compiled in chapter 2, a rather vulnerable evidential base. And it raises the unanswered question of why, if true, many other members of the common species were not swiftly subsumed by the new doctrine. While, therefore, Meeks has given an admirable account of the emergence of Paulinism, he tends to exaggerate the immediate social significance of his subject, not least because his theories involve an overdose of self-fulfilling prophecy.

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LUCIANO PERELLI. *Il movimento popolare nell'ultimo secolo della Repubblica*. (Historica, Politica, Philosophica, il Pensiero Antico, Studi e Testi, number 11.) Turin: G. B. Paravia. 1982. Pp. 257. L. 18,000.

"Noo, there are three reasons for taking the part o' the people: the first is, when general leeberty an' public happiness are conformable to your ain partecular feelings o' the moral and poleetical fetness o' things; the second is, when they happen to be, as it were, in a state of exceetability, an' ye think ye can get a gude price for your commodity, by flingin' in a leetle seasoning o' pheelanthorpy an' republican speerit; the third is, when ye think ye can bully the menestry into gieing ye a place or a pansion to hau'd your din." So Mr. Mac Laurel in Peacock's *Headlong Hall* (1815) in words which, with minor revision, might be applied to the politicians whom the Romans of the late Republic called *populares*. Luciano Perelli takes his definition from the celebrated and admittedly tendentious speech in which Cicero defended Sestius: *popularis* politicians are (to quote his translation) "coloro che volevano che fossero gradite alla moltitudine le cose che facevano e dicevano" (p. 26). Their opponents, the self-styled *optimates*, wanted, according to Cicero, to do what the best citizens would approve. This does not mean that Rome had a two-party system in the late Republic, as Mommsen thought. Nor does the interplay of ambition and alliance within the governing class provide a sufficient explanation for political aims and events. There were causes and grievances of a variety of groups outside the oligarchy that provided *popularis* leaders—who themselves came from the senatorial aristocracy or, if new men, from wealthy equestrian families (p. 13)—with motivation and support. It is Perelli's particular aim to define the intended beneficiaries of the bills proposed by *populares*, usually as tribunes, within the period 133–44 B.C. *Movimento* describes both the demands of the *populus* and the sporadic efforts of *populares* to meet them.

After an interesting survey of modern analyses and a useful but selective discussion of the contemporary and later ancient sources (he gives thorough consideration to Cicero's set-pieces in the speeches and philosophical works but little attention to the letters; discusses Sallust but no other late republican author, not even Caelius; concentrates on imperial historiographers and omits Horace and Vergil), Perelli in the second part and kernel of the book (pp. 73–227) attempts a "Traccia per la storia del movimento popolare." His narrative is lucid and his opinions judicious, but the scope of his work compels him to send his reader to other scholars on many topics (living conditions of the poor [p. 20], political vocabulary [p. 12], the *collegia* [p. 212]) and to concentrate his attention on the major leaders from the Gracchi to Caesar, to the neglect of new men and those who retire or change sides after their tribunate. The specialist will want to consult Perelli on particular points and, thanks to indexes of ancient and modern authors, can readily find his

balanced and occasionally scathing comments on his predecessors. The less-specialized reader will still need to go for a more comprehensive and original account to Peter Brunt's *Social conflicts in the Roman Republic* (1971).

In his conclusions, Perelli briefly sketches the groups who benefited from *popularis* legislation, the rural and city plebs, soldiers and veterans, Italians, *equites* (pp. 229–43). This chapter whets one's appetite for a more thorough exploration of a shorter period, for instance 70–50 B.C., which might combine the findings and advances in understanding achieved in the last century of modern scholarship, for example description of the *mentalités* and traditions of the masses as well as living conditions and sporadic crises, classification of legislation, prosopography of *popularis* legislators and their few recorded nonsenatorial supporters. That, since it would be necessary also to discuss *popularis* measures introduced by *optimates* and senatorial reaction to *popularis* measures, would amount to an analysis of the politics of the whole period. But the material for such a synthesis is to hand, thanks to the labors of scholars whose points of view differ widely from each others' and from Perelli's—Brunt, Gruen, Lintott, and Wiseman spring immediately to mind. Perelli's admiration for the programmatic article of Feliciano Serrao, "I partiti politici nella repubblica romana," in *Ricerche . . . C. Barbagallo* (1970), as well as his own preliminary chapter and conclusions, suggests the hope that he may go on from *histoire événementielle* to a deeper analysis.

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MEDIEVAL

DONALD WEINSTEIN and RUDOLPH M. BELL. *Saints and Society: The Two Worlds of Western Christendom, 1000–1700*. Chicago: University of Chicago Press. 1982. Pp. xii, 314. \$25.00.

The blessed Henry Suso once meditated on the symbolism of an apple in order to spur himself on to the imitation of Christ; now he is in an Apple—a fruit, that is, of the genus computer, which can accommodate millions of bytes. Although Donald Weinstein and Rudolph M. Bell have placed Suso there along with 863 other Christian saints who lived between 1000 and 1700 in order to pursue a macroscopic analysis of the interaction between saints and society, their study is not, as we might first expect, an assemblage of annotated tables. Quite to the contrary, the authors are just as committed to the literate analysis and exposition of their findings as any nonquantifier might be; indeed, they write

with great vividness and psychological penetration, integrate evocative contemporary works of art into their text, and even offer many arguments that do not rest on statistics at all. Ironically the result is that a troglodyte like the present reviewer occasionally wishes for less qualitative psychologizing rather than more.

The first half of *Saints and Society* attempts to draw conclusions about family history and personality development primarily from the qualitative interpretation of hagiographic narratives. Among the most prominent arguments that Weinstein and Bell advance in this regard are (1) that between 1200 and 1500 affective families were the norm, (2) that during the same period and beyond adolescence was recognized as "a distinct life stage," and (3) that medieval adults often underwent dramatic conversion experiences. Unfortunately, however, although any one of these propositions may well be true, the first and third can hardly be proven by reference to highly formulaic literary evidence unless a program is found for distinguishing reality from *topoi*. For example, did the thirteenth-century queen of Poland really nurse her child or was she merely said to have done so because the hagiographer wished to draw a comparison between her saintlike infant and Nicholas of Bari, who suckled from *his* mother's breast? As for adolescence, even if it is true that many saints opted for asceticism during their teenage years and came into conflict with their parents in so doing, it does not necessarily follow that the Middle Ages knew "adolescence" as a stage of personality development.

On the other hand, the treatment of society's perceptions of sainthood in the book's second half finds the authors on much firmer terrain. Quantifying their hagiographic evidence to determine when and where saints coming from one or another social background and belonging to one or another clerical order or the laity were perceived to have been endowed with one or another saintlike characteristic, Weinstein and Bell conclude convincingly that the "years from about 1200 to about 1540 . . . emerge as a coherent period in the history of Catholic piety" because "this was the period when conversion [became] a conscious choice. . . when the female contemplative came into her own, when the saint's favored place [became] the town . . . [and] when lay people actively joined the pursuit of spiritual perfection" (p. 102). Among numerous impressive statistics supporting this interpretation are the doubling of female saints from the twelfth to the thirteenth century and the increase of laypeople and tertiaries among saints in the same hundred years by a factor of over 50 percent. Because the authors go very far toward explaining such phenomena rather than merely recording them, the result is highly recommended reading for all students of

medieval and early modern religion as well as a strong recommendation for the canonization of St. Silica.

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ANTONIA GRANSDEN. *Historical Writing in England*. Volume 2, *C. 1307 to the Early Sixteenth Century*. Ithaca: Cornell University Press. 1982. Pp. xxiv, 644. \$75.00.

With the publication of this volume, Antonia Gransden has completed her monumental survey of historical writing in England from ca. 550 to the early sixteenth century. Anyone interested in the narrative sources for the history of medieval England now has a reliable guide, and at least a generation of scholars will be in her debt. Volume 2 is topic-oriented and has chapters dealing with the narrative sources for the reigns of Edward II and III, Richard II, Henry V, the Wars of the Roses, London, the end of the monastic tradition of historiography, antiquarians, and humanist historians. Separate chapters are devoted to Higden and to Thomas Walsingham. There are eight appendixes, an inadequate bibliography, and an inflated index of 125 pages.

Gransden sets herself the task "to examine the motives which impelled men to take up again the burden of historical enterprise, and to map the roads along which they traveled" (p. xi). She seems far less concerned, however, with the motives of individuals than with making clear to the reader the value of a particular work as a source for the writing of medieval history by modern scholars. Motives seem to be explored generally in light of the way in which they may have led an author to distort the "truth" and thus provide an unwary contemporary researcher with unreliable information. Gransden's final observation—"Probably the medieval chroniclers' gift for contemporary reportage . . . taught little of value in technique: it did, however, bequeath . . . to posterity an inexhaustible store of information . . . without which the history of medieval England could not have been written"—is the most reliable guide to her attitude and purpose (p. 479).

Unlike the first volume, which lacks a thesis (see my review *ante* 1977), volume 2 traces several trends of interest. For example, it is persuasively argued that the fourteenth and fifteenth centuries saw the general decline of monastic chronicles as the dominant genre as members of the secular clergy and then laymen (both bourgeois and noble), respectively, came into prominence as writers of historical works. This process parallels a change from writing in Latin to Anglo-Norman and then to English.

Little is said in this context, however, as to what these trends mean in terms of an overall understanding of the history of medieval England.

In a more particular vein, I should like to call attention to a fallacy frequently indulged by medievalists when dealing with narrative sources: Gransden writes that Polydore Vergil "grasped the basic principle that sources written closest in time to the event recorded are usually of more authority than later ones" (p. 436). If, by authority, Gransden means accuracy, it should be emphasized that no evidence of a statistical nature has been adduced that proves closeness in time to events is the basic variable in assuring veracity. Like so many other historians' shibboleths this one has been adapted uncritically from lawyers whose goal is victory not truth and to whom "basic principles" or rules such as the above are substitutes for research.

Let me leave the reader with a negative impression let me call attention once again to Polydore Vergil, whom Gransden quotes: "History 'is the only unique, certain and faithful witness of times and things, redounding as much to the glory of the author as to the usefulness of posterity'" (p. 434). *Historical Writing in England* teaches us much about times and things, its usefulness to posterity is unquestioned, and there is even glory for its author.

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A. R. BRIDBURY. *Medieval English Clothmaking: An Economic Survey*. (Pasold Studies in Textile History, number 4.) London: Heinemann Educational Books. 1982. Pp. xiii, 125. \$27.50.

Attempts to write the history of the medieval English cloth industry have faced serious problems. Records are few, and some of these, the aulnage accounts, are untrustworthy. Conclusions must be based on what seems reasonable rather than on any certainty. The standard account has been that of Eleanor Carus-Wilson, most succinctly presented in the second volume of *The Cambridge Economic History*. Because of the establishment of numerous fulling mills in Wales and western England during the thirteenth century, Carus-Wilson concluded that clothmaking moved westward from the towns on the eastern seaboard. Simultaneously, clothmaking became a rural rather than an urban occupation. One exception seems to have been Leicester, whose voluminous records for the period led Carus-Wilson to rate that town as one of the leading clothmaking towns in the realm. The shift from wool exports to cloth exports in England from the fourteenth century onward was ascribed to high export duties on wool balanced by low tariffs on cloth. Foreign buyers

of English wool would consequently have been at a disadvantage.

It is these main conclusions that A. R. Bridbury challenges, depending rather more on speculation than Carus-Wilson did. Concerning the fulling mills, he finds their location less compelling on the supposition that the eastern areas may have found fulling by foot more convenient. As for the shift from urban to rural production, he considers Leicester unique in the preservation of its records, not in its cloth output, affirming that, if other towns had been so fortunate in the survival of their records, Leicester's case would be the norm and not the exception. For the fourteenth century he offers the example of Salisbury as an important urban center of clothmaking. Not merely a producer of cloth, it was a center for cloth trade with the majority of its foreign cloth, according to Bridbury's account, coming from the southwestern counties. Both its location and its trade support Carus-Wilson's contention of the westward movement in cloth production. The most effective part of this brief work demonstrates that high export duties on wool had no immediate or long-term effect on England's strength in foreign markets. Wool exports remained considerable, while cloth exports rose only gradually, not really rising rapidly until the last third of the fifteenth century. Possibly, also, when English wool went into permanent decline as an export commodity from the 1450s, foreign clothiers were turning to the cheaper Spanish wool. Bridbury seems to be right in denying that fiscal policy or mechanization of production (fulling mills) accounted for this swift advance in the fifteenth century. He ascribes it, probably correctly, to the English being better craftsmen, designers, and businessmen who responded well to changing fashions. The chief weakness of the work is its dependence on negative evidence.

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HÉLÈNE MILLET. *Les chanoines du chapitre cathédral de Laon, 1272-1412*. (Collection de l'École Française de Rome, number 56.) Rome: École Française de Rome. 1982. Pp. 548.

Endowed corporate bodies played a major role in medieval religious life as well as in economic and political matters. In modern times the most carefully studied of such corporate bodies have been monasteries, perhaps because their successes and failures in leading a utopian lifestyle had such repercussions on their contemporaries. The numerous secular cathedral chapters have suffered from a relative lack of scholarly interest. Hélène Millet has demonstrated how informative a study of such a chapter can be.

The cathedral chapter at Laon was the largest in fourteenth-century France (83 canons). The author based her research on twenty complete lists of canons drawn up periodically between 1272 and 1388. In addition, she had the minutes of chapter meetings from 1407 to 1412, the information in the papal registers, and other archival material. The author was able to gather an imposing mass of biographical detail on the canons (52,000 pieces of information), which she organized and analyzed with a computer. One strength of the monograph is her full explanation of the use to which she put the computer. Her intelligent and careful exploitation of the sources permitted her to study the entire membership of a great canonry over a period of 140 years and not merely the few canons who were prominent in their time. The result is an illuminating group portrait of the 850 men who were canons of the cathedral chapter of Laon from 1272 to 1412.

In part 1, the author analyzes the geographical and social origins of the canons as well as their education and activities outside the chapter. Few generalizations emerge, and that is a strength of the study, which respects the complex reality it encounters. Because the popes had a firm control of appointment to the chapter, the recruitment was rather cosmopolitan. Members came from northern France, Italy, and, after the popes settled at Avignon, southern France. The canons were not predominantly nobles, but they were generally recruited from the upper strata of society. A high proportion were also recommended by university degrees. In 1409, 86 percent of the canons were university men. Diversity was the norm: the author suggested that the only common feature was that each canon was a cleric with a good education.

In part 2, Millet assesses how a candidate actually became a canon. By the fourteenth century, the papacy appointed successors to vacant benefices, although pressure from clerics and their patrons influenced the decisions. This monograph provides a local perspective on the functioning of the fourteenth-century papacy. The author was able to link shifts in recruitment to changes in the papacy and its policies.

In part 3, she pieces together the scanty evidence on the lifestyle of the canons. The author appended 134 biographical notices on individual canons and 50 biographical notices on office holders in the chapter.

The canons emerge in their group portrait as sober, respectable people. They were socially conservative, jealous of their personal and corporate privileges, and conventionally pious. Order and legality reigned and scandal was quite unusual, but so was exceptional fervor. It is striking that the cathedral chapter had been stripped of most of its functions. The papacy chose the bishop of Laon as

well as the canons. The canons no longer served as the bishop's counselors, nor did they perform regular pastoral or educational work. Their sole corporate duty was to guarantee the liturgical services in the cathedral. In addition, they expended much energy to manage and defend their corporate wealth.

This meticulous local study illuminates an important facet of church life in fourteenth-century Europe. The cathedral chapter was a force for stability, order, respectability, and the status quo. The power of such institutions helps explain the inability of the late medieval church to reform itself and to adapt to changed conditions.

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JOACHIM BUMKE. *The Concept of Knighthood in the Middle Ages*. Translated by W. T. H. JACKSON and ERIKA JACKSON. (AMS Studies in the Middle Ages, number 2.) New York: AMS. 1982. Pp. viii, 268. \$29.50.

This is a translation of the considerably revised, 1977 edition of a book that was originally published in 1964. In the second edition Joachim Bumke commented on subsequent research and responded to his critics. As the publication of the first edition as a supplementary volume to *Euphoriion: Zeitschrift für Literaturgeschichte* revealed, the book was intended for students of medieval German literature. Bumke's study of the concept of knighthood was based on an analysis of Middle High German texts, supplemented, where appropriate, by the findings of historians. Bumke too modestly demurred that he was not telling historians "anything new" (p. 7), but most American medievalists who are unfamiliar with German social history will profit from Bumke's synthesis of the literature.

Bumke attacked the widespread belief among Germanists that the knights of the Hohenstaufen period shared a common, lay, chivalric culture that found its expression in poetry and belonged to a single class of heavily armed cavalymen that embraced everyone from the emperor to a ministerial. It is a view familiar to anyone who has read Marc Bloch's *Feudal Society*. Bumke demonstrated that the word *knight* continued to be applied to warriors in general as well as the military elite and that the essence of the *Ritterbegriff* was service, whether on the battlefield or at court, rather than fighting on horseback. The real question is why a term with such connotations was adopted by the nobility as an honorific at the end of the twelfth century. The best explanation is that the concept of the *miles Christi* ennobled the idea of service. This does not mean that the lowborn knight was ever perceived as the

social equal of a noble; indeed, *miles* became the standard designation in the thirteenth century in southeastern Germany, where much of the courtly literature was written, for the unfree retainer of a ministerial. It was the word that rose in value, not the people who were described as knights. German chivalric culture was not the creation of upwardly mobile ministerials; it was the princes who adopted the latest French fashion, and the *Dienstmänner* adopted it as they were assimilated into courtly society.

The weakness of Bumke's book is the basic failure of medieval German social history. German medievalists have been adept in evaluating legal, literary, philological, and genealogical evidence; but, in spite of the popularity of *Landesgeschichte* since World War II, a study of the long-term social trends in an individual territory, comparable to Georges Duby's classic work on the Mâconnais, has yet to be written, as Bumke himself points out (pp. 129-32). Until it is, all discussions of medieval German social history will retain a certain abstract quality. The book is a pastiche of quotations from primary sources and secondary literature and the Jacksons' decision to include both the original Middle High German and an English translation, while probably necessary, has added to the difficulty of reading the book. Nevertheless, they are to be commended for making this important book available to a larger audience.

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RANDOLPH STARN. *Contrary Commonwealth: The Theme of Exile in Medieval and Renaissance Italy*. Berkeley and Los Angeles: University of California Press. 1982. Pp. xix, 207. \$24.50.

"Contrary Commonwealth" refers to the imaginary as well as physical space inhabited by exiles, citizens compelled to live outside their homeland. It stands as the logical opposite of the much-studied and more-admired civic world of republics and courtly life of principalities. Recent studies of thirteenth-century Siena by Pazzagliani and of Renaissance Venice by Cozzi remind us that banishment and exile were punitive measures frequently and effectively employed against debtors, tax delinquents, malefactors, and political opponents. These studies also reveal that the archival sources for the study of banishment are extremely rich. The easiest way to tap these sources is through regional studies; the more complex and daunting, but certainly more fruitful, way is through systematic comparative study. Rejecting these approaches, Randolph Starn has taken a third approach that touches briefly and insightfully on myriad aspects of what he calls "the theme of exile in medieval and Renaissance Italy."

The portrait drawn by Starn is familiar. In the thirteenth and early fourteenth centuries there were thousands of exiles organized in large companies roaming the Italian peninsula. In 1323 there were at least 4,000 persons in exile from Florence and 1,500 from Bologna. These exiles were the products of factional rivalry, protracted Ghibelline-Guelph strife, and antimagnate legislation. Exiles thrived in the fragmented political landscape of late medieval Italy, posing a constant threat to the security and stability of their homelands. Exiles did not thrive as well in Renaissance Italy, with its centralized, territorial states. Relying on summary procedures, expediency, and administrators tied to the ruling regimes, these states effectively quashed internal dissent and pacified opposition in the territories they had conquered. Outside the territories, the behavior of exiles was now monitored by resident ambassadors. The large-scale campaigns waged by companies of medieval exiles gave way to scattered violence, diplomatic intrigue, and petty sabotage.

Starn's neo-Burckhardtian portrait is not without flaws. Almost no attention is paid to the lower orders of society, not enough to debtors and tax delinquents, who most likely comprised the bulk of citizens placed under the ban, while too much attention is paid to Florence, whose unique history is not a reliable index for what occurred elsewhere in Italy. There are several notable bibliographic omissions: Mario Sbriccoli's Marxist account of *Crimen Laesae Maiestatis: Il problema del reato politico alle soglie della scienza penalistica moderna* (1974); Giorgio Zordan's *Il diritto e la procedura criminale nel tractatus de maleficiis di Angelo Gambiglioni* (1976); and E. Ludwig Grasmück, *Exilium: Untersuchungen zur Verbannung in der Antike* (1978). Finally, modern metaphors such as "guerilla warfare," "pacification," and "fifth column" make for lively reading, but they subtly prejudice the reader against exiles as reactionary enemies of the inevitable triumph of the modern state, who deserved their fate. Despite these flaws and omissions, Starn's book represents an excellent and elegant introduction to the subject. Especially valuable is the final chapter, a fine evocation of the voices of exile in the literature of medieval and Renaissance Italy.

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JOHN M. NAJEMY, *Corporatism and Consensus in Florentine Electoral Politics, 1280-1400*. Chapel Hill: University of North Carolina Press. 1982. Pp. xiv, 344. \$28.00.

Work in Florentine history, at least insofar as American and British scholars are concerned, moves backward: from the struggles of adherents of a

republican form of government against the establishment of a Medicean dynasty back to the brilliant times of Cosimo Pater Patriae and Lorenzo Magnifico and then back to the turbulent decades in which the Medicean oligarchy arose. John M. Najemy takes a further step backward: the work begins in the thirteenth century and stretches over the entire fourteenth century. His important book is the result of careful and thorough research, and we have now an up-to-date scholarly basis for further discussions of Florentine history in the Trecento.

This can be said although the theme of the book is limited. The task that the author has set himself is to provide a "history of the Florentine guilds and guild politics in the fourteenth century," and in the work under review he is concerned with one particular aspect of guild politics: the involvement of the guilds in the election of office holders. The electoral process, however, was so crucial for Florentine political developments that Najemy's study becomes a history of Florentine domestic policy in the fourteenth century.

This century opened with the guilds dominating Florentine politics and determining the selection of the ruling magistrates. Then their oligarchic opponents—a combination of rich merchants, big entrepreneurs, and magnates—recovered and attained control. The rest of the fourteenth century was filled with the struggle of the guilds to regain influence in the selection of the Florentine government and with the political efforts of the oligarchs to ward them off. This conflict was reflected in continuous changes in the regulations that determined the groups of society that had a right to participate in the elections. These changes were frequently very technical and expressed in a complicated language; Najemy has done admirable work in analyzing and explaining the evolution of these difficult regulations. A crucial step in their development was the reform of 1328; it introduced the drawing up of an extended list of nominations, which were then subjected to a scrutiny in which the guilds succeeded in having a part, although only a minor one. A comparison of this reform with the Venetian *serrata* might have been instructive because both "reforms" had a tendency to create stability by broadening the circle of privileged citizens and at the same time closing this circle. Such comparisons are not popular, however, among students of Florentine history because the inexhaustible richness of the Florentine archives makes it difficult for them to turn their eyes, if only briefly, to other cities of the Renaissance. Still more important than the reform of 1328 was the reform of 1352 because it resulted in stability for a lengthy period; the guilds achieved participation in the nominating process, although this extension of guild influence was somewhat balanced by the addition of a large number of

appointed members to the scrutiny committee. This compromise broke down in the crisis caused by the war against the papacy and by an economic decline; it led to conflict within the ruling oligarchy and sharpened the tensions within the guilds: between the major and the minor guilds and between employers and employees.

Najemy's treatment of these years adds to our understanding of the Ciompi Revolt; his analysis of the frequent changes in electoral procedures shows how the discontent in the guilds was slowly and gradually building up and increasing. The defeat of the Ciompi then established the oligarchy firmly in power; the list of eligibles continued to be kept rather broad, but a preferential pool was created among them. This satisfied "the general thirst for participation in politics" and left the crucial selection to the small, powerful group of oligarchs.

Najemy's investigation has now reached those decades of Florentine history that in recent years have been studied by Gene Brucker, Dale Kent, Anthony Molho, and Ronald Witt, and, appropriately, Najemy's work ends with the appearance of the Florentine *deus ex machina*: civic humanism. I doubt, however, that Bruni's funeral speech on Nanni Strozzi can serve as proof that a new political ideology, focused on equality and justice, had then appeared in Florence. This speech was a paraphrase of Pericles' funeral oration in Thucydides, and Bruni was more concerned with showing that Florence was Athens reborn than with expressing the political ideas of his time.

Najemy's thesis is that a new period of Florentine history could begin in the fifteenth century because the consensus on which the oligarchs relied had overcome the corporatism that the guilds represented. The notions of "corporatism" and "consensus" that give the title to Najemy's book might be useful for periodization of Florentine history, dividing the fourteenth from the fifteenth century. But were those who fought for controlling influence in Florentine affairs in the fourteenth century conscious of representing different and antagonistic principles of government? So many different factors—external events as well as economic developments—spurred the internal struggles that the contrast between corporatism and consensus appears to me a useful later conceptual construction rather than a fight for opposing political principles of which the Florentines of the fourteenth century were aware and around which they rallied. Najemy seems to me to abbreviate the road from interest to ideology. Fortunately he intends to continue his investigation of Florentine guild politics, and he will then probably answer this question, which at present seems to me left open.

My remarks do not mean to deny the significance of the change from the Florentine fourteenth to the

fifteenth century. It is characteristic that the evolution of the electoral procedures that in the fourteenth century had been crucial in revealing the tensions and shifts in political power had now come to an end; the regulations that were established at the end of the fourteenth century remained in force until the sixteenth century. Students of Florentine history of the fifteenth and sixteenth centuries frequently have difficulties in understanding the complex and clumsy machinery by which the Signoria and the magistrates were elected. It is no small merit of Najemy's book that he has presented with great precision how the various parts of this machinery—the establishment of nominating committees, the drawing up of a list of candidates, the examination (scrutiny) of these lists, the placing of the names on the lists into bags, and, finally, the extraction—were introduced and shows the reasons for the existence of each of the links in the proceedings.

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NORMAN GOLB and OMELJAN PRITSACK. *Khazarian Hebrew Documents of the Tenth Century*. Ithaca: Cornell University Press. 1982. Pp. xvi, 166. \$38.50.

Because so few sources by or about the Khazars have survived, scholars have offered widely varying assessments of them. The Khazars were a Turkic people who flourished between the seventh and tenth centuries in what is today the southern USSR. But how should one characterize their early relationships with the Rus? And did many Khazars convert to Judaism, or did only a few of their kings and grandees embrace that faith?

Norman Golb and Omeljan Pritsak now present two Hebrew texts that, when combined with additional evidence, help answer these and other questions. Their book includes photocopies of both manuscripts, along with printed Hebrew texts of the same documents, accompanied by Golb's new English translations on the opposite pages. Paleographic and textological analyses follow, which argue convincingly in favor of both texts' authenticity. Each document is then fitted into a broader historical background, to produce most interesting conclusions.

The first manuscript, referred to as "the Kievan letter," is "the earliest original document in any language containing the name Kiev" (p. 21). It was discovered near the end of the nineteenth century among manuscripts in a *genizah* (storage room) of an Egyptian synagogue. Written by Kievan Jews (some with names betraying Khazar origin) to other Jewish communities in Egypt, the letter appeals for money to help a Kievan Jew pay off the debts of his brother,

slain by brigands. The editors believe it to date from the first half of the tenth century (ca. 930 A.D.). We cannot repeat the editors' arguments in this brief review, but find their thesis plausible: "Khazars who adopted Judaism . . . came to form a part of the Ukrainian component of eastern European Jewry . . . eventually to be assimilated by it" (p. xv). This is an intermediate view, between the claim that only a few, high-ranking Khazars adopted Judaism and the opposing hypothesis that large numbers of Ashkenazim are descended from Khazars. The text also provides data on such topics as early suretyship and moneylending practices.

The second document—known as the "Schechter text" (an anonymous Khazar's epistle to Hasdai ibn Shaprūt of Cordova)—is a fragment dating from the mid-tenth century. Its authenticity has earlier been questioned, but Golb and Pritsak make convincing arguments that it is genuine. The fragment describes the flight of Jews "from or through Armenia" to Khazaria, where they were well received. After a disputation over faith between Jewish, Muslim, and Christian spokesmen, the Khazar chieftains chose Judaism. The consequences were profound: "the institution of Khazarian Kingship is . . . intrinsically connected with the conversion of the Khazars to Judaism" (p. 103). The document claims that other Jews flocked to Khazaria from "Baghdad and from Khorasan, and from the land of Greece, and they strengthened the men of the land" (p. 111). Under various kings "the fear of the officers of Qazaria was over the nations around us" (p. 113).

Both texts bear directly on early Russia, yet some readers will find Pritsak's commentary mind-boggling. The Khwārizmian name Kiev itself is "politically and culturally . . . a Khazarian (Kabar and Onogurian) element" (p. 55). The "Kievan Kabars/Kopyrs were Jews by religion"; not only was there a "Jewish gate" in Kiev but a whole Jewish suburb (the *Kopyrev konec*) (p. 57). So far so good. But Pritsak then argues that the Rus had not yet conquered Kiev when the "Kievan letter" was written; the Khazars were still lords of the city in 930 A.D. Certain well-known chronicle accounts of Oleg's conquest of Kiev in 882 A.D. are patent myths: the Schechter text proves that, far from being a wily, all-conquering epic hero (as he appears in most histories of Russia), Prince Oleg was "soundly beaten," first by the Khazars and then by the Greeks, against whom he had been compelled by the Khazars to launch a reluctant naval campaign. Only later (shortly before 941 A.D.) did the Rus, under Igor, capture Kiev.

Golb's analysis (in "Section A" following each text) should meet with only scattered opposition, but Pritsak's theses (in "Section B" following each text) may well result in controversy—or be ignored by

persons daunted by Pritsak's formidable erudition yet committed, for whatever reason, to more traditional views of early Russian history.

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MODERN EUROPE

RALF DAHRENDORF. *On Britain*. Chicago: University of Chicago Press. 1982. Pp. 200.

This is a book for a train journey, not least because of the Cook's Tour nature of its contents. Somehow one feels uneasy when wandering scholars—from Karl Moritz of the eighteenth century to M. J. Bonn of the early twentieth—choose to dilate on the life, institutions, and society of their adopted countries. In a similar fashion Ralf Dahrendorf discusses the strengths and weaknesses of Britain against the background of its relative economic decline. But it has to be said that many of the strengths he records—continuity, tradition, and solidarity—can be regarded as weaknesses in terms of economic vitality. Moreover, it is not clear how some of the author's suggestions—such as electoral reform, a bill of rights, and a national forum to debate the future of the economy—would help to rejuvenate a flagging economy. They might go some way toward securing greater stability, but this in itself would not necessarily generate growth and prosperity. Rather, what is needed is a greater awareness and appreciation of the importance of the profit motive. It is probably in this respect that our educational institutions fail by training an educated elite that despises business and the profit motive. How many graduates, for example, would be eager to join one of Britain's most successful companies, BTR, which unashamedly boasts that "the single-minded pursuit of profit should be the driving force of British management"?

This is the secret of success, but, unfortunately, there are not enough such performers in the country to ensure Britain's economic viability. There is some evidence, however, that during the last few years things have been changing for the better. Dahrendorf perhaps underestimates the progress made as a result of the process of structural transformation. On economic matters generally, the author is not very perceptive. One cannot, for example, reduce inflation without creating unemployment in the short-term whichever route one takes, but much of the current unemployment is a product of the structural transformation process now taking place in mature economies and cannot be cured quickly.

Dahrendorf has some interesting points to make on Britain's economy and society, but most of them have been said before. He ranges over a wide area in a rather cursory manner without providing any real answers to Britain's problems.

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BARBARA KANNER, editor. *The Women of England from Anglo-Saxon Times to the Present: Interpretive Bibliographical Essays*. Hamden, Conn.: Archon Books. 1979. Pp. 429.

The Women of England, edited and introduced by Barbara Kanner, consists of eleven bibliographical essays followed by bibliographies of both primary and secondary sources. Arranged chronologically, the volume begins with the women of Anglo-Saxon England and concludes with a discussion of the primary sources for the study of twentieth-century Englishwomen. Kanner's introduction provides an informative discussion of trends and controversies in the methodology of women's history. The collection is a teaching and learning guide, suggesting approaches and titles to the beginning investigator.

The sins of the book are all of the lesser sort; not those of commission but of omission. As several of the contributors themselves assert, "there is a real dearth of secondary writing . . . especially in the neglected area of economic life" (p. 85). Thus, the women examined are those, by and large, who left records or were described in the obvious sorts of historical sources, that is, journals, novels, letters, and so forth.

At some places in the book I found myself yearning for nonexistent essays. How can one explain the absence of a piece on the effects of changes in the law on women in nineteenth-century England? An essay on women and war would also have been welcome. Women, after all, contributed significantly to the effort and outcome of at least three of England's major wars. I was also disappointed not to meet some of my favorite historical women in this collection. Sarah Fielding, sister of the more famous Fielding brothers, Henry and John, does not appear, although she was an important novelist of her day. Neither does Lady Morgan, author of the early nineteenth-century bestseller *The Wild Irish Girl*, who shocked contemporaries by appearing in the Ladies Gallery of the House of Commons in trousers.

Having praised *The Women of England* as a teaching guide, I must, however, state that in some important ways it is incomplete. Published in 1979, the volume could not take note of the wealth of material subsequently written. Much of this newer writing attempts to fill in some of the gaps evident in *The Women of England*. Keith Snell's work ("Agricul-

tural Seasonal Unemployment, the Standard of Living, and Women's Work . . . 1690-1860," *EHR*, 34 [1981], for example, uses settlement examinations to illustrate the nature of women's involvement in agriculture and adds to our understanding of the daily lives of ordinary women. Iris Minor's work ("Working Class Women and Matrimonial Law Reform, 1890-1914" in David E. Martin, ed., *Ideology and the Labour Movement*) looks at the effects of law reform on the lives of working women, while John Gillis's massive study of the mothers of foundlings in nineteenth-century London ("Servants, Sexual Relations, and the Rise of Illegitimacy in London, 1801-1900," *Feminist Studies*, 5 [1979]) illuminates the connection between class, sentiment, and sexual expectations. Again, I am not faulting the contributors to *The Women of England* for not including these more recent works—that would have required foresight of a suprahistorical sort. What I hope, as may most readers of *The Women of England*, is that Kanner will soon be coming out with a new, revised, and enlarged, "bigger and better than ever" edition of this fine book.

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LINDA LEVY PECK. *Northampton: Patronage and Policy at the Court of James I*. Winchester, Mass.: Allen and Unwin. 1982. Pp. x, 277. \$37.50.

After years of conspiracy, disrepute, and straitened circumstances, Henry Howard gained partial acceptance at Elizabeth's court. Soon James VI made him the link in correspondence between Edinburgh and Robert Cecil, who could thus prepare for the future with a hatchet man to hand. In 1603, the sexagenarian attained that recognition and place to which he felt entitled by lineage: earl of Northampton, privy councillor, and lord warden of the Cinque Ports. Five years later he acquired the Privy Seal, a faded office relinquished by Cecil. These positions were to be the basis of his own patronage and meant the acquisition of that political influence and material reward that he also assumed to be his due. If deference to men of lesser birth, notably Carr, was a concomitant of this pursuit, it was after all royal patronage of James's kind, not family status, that had opened the door for Northampton. In any case, the king seems to have vested considerable confidence in him, not least as a spokesman in the sessions of the first Parliament. On grounds of both performance and policy, however, James's trust waned once Cecil was gone. An ailing Northampton, in the weeks prior to his own death in 1614, reexperienced the sensation of being counted as negligible.

Since only Northampton's years of eminence are

under review, Linda Levy Peck's offering is less a biography than a chain of articles. These are thematic, with the emphasis on administration and patronage, Parliament and politics. This structure provides scope for ample comment on, for example, foreign policy and her subject's private and public attitudes toward Roman Catholicism. The result is neat and informative. Only occasionally, as with the Cockayne project, is some disjunction incurred.

In contrast to his legendary reputation, Northampton is presented as a reformer. Here care must be taken not to strain the evidence, but, as Peck points out, personal interest was not incompatible with concern for the crown's administrative and fiscal stability. Nonetheless, it is a fact that the latter concern degenerated—not an uncommon occurrence among court politicians in general. On another matter, it is demonstrated that Northampton foresaw that improper preparation would undermine the 1614 Parliament. He was not present and no longer attended the Council; but his name was to be associated not with arguments about whether the Parliament should meet but with the fact of its dissolution. At the time there were many genuine arguments among royal advisers, but two decades or two centuries later anyone who was deemed to have opposed a Parliament was automatically regarded as malign.

This book is deft and valuable. It takes perspicacity to restore the character of Northampton in such a way that he emerges in his new and improved light as a cynical, pedantic, and proud personage, who combined flattery with resentment. Peck's quality is in providing explanation, not defense; and the man is portrayed within his context. Northampton no longer quite fits the criteria that Raleigh attributed to men of high condition, and the geriatric rogue has been replaced by an earnest but starchy trimmer.

W. J. JONES
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TIMOTHY GEORGE, *John Robinson and the English Separatist Tradition*. (National Association of Baptist Professors of Religion, dissertation series, number 1.) Macon, Ga.: Mercer University Press. 1982. Pp. ix, 263.

Originally a doctoral dissertation to the faculty of Harvard Divinity School, this erudite study of John Robinson will be an important contribution to our understanding of the theological or doctrinal dimensions of Robinson's Separatist ecclesiology. To be sure, celebrated as a leading exponent of Separatism in the Puritan movement of early Stuart England and especially as the pastor of the Pilgrim Fathers at Leiden, Robinson has been a well-known

figure in history. Yet the traditional portrayal of Robinson has often left untouched and unrevealed his deeper doctrinal concerns. As Timothy George has rightly argued and convincingly demonstrated in this book, "the Separatist phenomenon was not merely the most radical form of Puritan protest against the Church of England but also a distinctive quest for a new sense of Christian community" (p. vii). It was Robinson in the early Separatist tradition who was "to place the Separatist concept of the visible Church within an overarching context of predestinarian theology" (p. 170). These underlying themes are well chosen; for, insofar as religious movements in history are concerned, a theological approach is truly a historical one.

Besides a brief introduction and a succinct conclusion, this study consists of four main chapters, two historically oriented and two theologically structured. Chapter 2 is a historical account of the English Separatist tradition from the Kentish conventicles in the Edwardian era to the rise of the more famous Separatists—Robert Browne and Robert Harrison, Henry Barrow and John Greenwood—during the Elizabethan period. Two points in this chapter are worthy of note: there was, indeed, a historical lineage, spiritual if not institutional, between the early Separatists and later Separatist churches in England; and the contributions of Robert Browne to Separatist ecclesiology and the concept of covenant are unmistakable. It is a short but informative account, and George has a perceptive eye for telling quotations. The following chapter on John Robinson is not merely a biographical sketch but rather a study of the historical context in which the theological mind of Robinson was formed and his Separatist position was finally developed. There is an absorbing discussion on the influences of William Perkins and the Calvinist—proto-Arminian controversy in the 1590s at Cambridge. This period constituted a watershed in his intellectual development: "Robinson drank deeply from Perkins' well" (pp. 62, 68); and he was to carry the latter's Calvinist theology into separation. Although the road to Separatism was tortuous, Robinson's sermon at Norwich in August 1603 already pointed to his future destiny. "For Syon's sake he must speake and for Syon's sake he would not holde his tongue" (p. 70). How true do these words ring of the fundamental mentality of all the Separatists in early Stuart England!

The remaining two chapters deal, respectively, with Robinson's biblical "justification of Separation" and his theological exegesis of the "mystery of election." No synopsis, however, can be made for either of these two chapters without doing great injustice both to the scope and to the nuances of George's careful analyses and interpretations. Suffice it to say that in the former the analysis is mainly

based on Robinson's *A Justification of the Separation from the Church of England* (1610) in the light of Lutheran and Calvinist concepts of the locus of the true church, whereas in the latter it is derived chiefly from Robinson's *Defence of the Doctrine Propounded by the Synode at Dort* (1624) in the context of the Arminian dispute at the University of Leiden. And George writes with a clarity that would delight a reader without theological training.

In conclusion, John Robinson appears in this study as a Separatist and Calvinist *par excellence*, whose concept of ecclesiology was at odds with both Anglican and Puritan models (pp. 116, 166). Although George is not unaware of the difficulty in maintaining a rigid demarcation between the Separatist and the Puritan (p. viii), such a distinction appears to have been maintained in this study. But what was an Independent, as Hugh Peters would later say, "but an overgrown Puritan" (Thomas Edwards, *Gangraena*, p. 183)? During the Interregnum, when the Independents were in power, many leaders of the Separatist churches in London, lay as well as clerical, took part in the ministerial and civic affairs of the city's parishes.

TAI LIU
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ROBERT W. MALCOLMSON. *Life and Labour in England, 1700-1780*. New York: St. Martin's Press. 1981. Pp. 208. \$22.50.

As the title suggests, this is a study of work and social life in eighteenth-century England. The book is about the laboring classes and is designed to reconstruct their experiences in four main areas: making a living; the role of the family; beliefs, customs, and identities; and authority, legitimacy, and dissension.

Robert W. Malcolmson recreates a world in which the majority of people lived in rural areas and took sustenance from a number of sources, including a variety of customary practices. People depended on a family economy in which women and children, as well as the men, worked. For most of the laboring people the family was the central unit of social experience and personal relations, including the phases of development from childhood to adulthood and coping with the hazards of life. Popular culture, the world of customs, rituals, values, mores, beliefs, and identities, provided the means of understanding and interpreting the pleasures and hazards of laboring life. With little regard for the models of cause developed in the physical sciences, popular culture expressed a profound belief in the power of supernatural forces. Beyond their own immediate lives, the laboring people sustained relations with authority. Malcolmson here largely accepts E. P. Thompson's theses about alternative definitions of

social reality, the negotiated quality of deference and obedience, and the confrontations of market and moral economy.

The analysis that supports these conclusions is static, structural, and descriptive. The organization of the chapters may be explained in part by the principle set out in this passage: "Man has needs which must be satisfied if life is to continue; he procures, through labour, the means of survival; and the terms under which this subsistence is attained profoundly condition much of his culture and social experience. Getting a living, then, is the foundation on which social life is built" (p. 22). But how, in detail, is the business of getting a living linked to the problems of culture and interaction with authority? In what way does getting a living cause, determine, or direct consciousness and behavior? An answer must go beyond the static and structural to deal with the interweaving of the social, economic, and political. Malcolmson's response to this problem has been to put questions about change into a final chapter that deals with landlords, laborers and wages, social relations, and standards of living. The result, though, hardly integrates the structural and descriptive material of the previous chapters into an analytical whole. The best and fairest comment that can be made is this, in the author's own words: "A social reconstruction of the relatively stable, continuous and recurrent dimension of experience is an important and often essential aspect of the historian's task, but this bias towards a static analysis . . . cannot be allowed to remain largely dissociated from the attempt to understand the dynamics of a society. Let us turn, then, to concentrate on these questions of social change" (p. 135).

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BARRIE TRINDER. *The Making of the Industrial Landscape*. London: J. M. Dent and Sons; distributed by Biblio Distribution Center, Totowa, N.J. 1982. Pp. xi, 267. \$24.95.

Two books more than any others have shaped our understanding of the history of English landscape: W. G. Hoskins's *The Making of the English Landscape* and Maurice Beresford's *History on the Ground*—even if Hoskins is rather unsympathetic to the landscape of industry and if in that particular book Beresford does not deal with strictly industrial subjects. But indirectly, both made an important contribution to historical reassessments of the timing and impact of industrialization by pointing to a significant proto-industrial phase predating the classic take-off of the later eighteenth century. While Barrie Trinder inherits much from Hoskins and Beresford, his

approach is historical, not geographical, and he seeks to explain the phases of industrial development by recreating the landscapes of the Industrial Revolution. In a lively text punctuated by contemporary accounts he reactivates furnaces, machines, and steam engines; repopulates back-to-back terraces; sets barges, horse gins, and steam locomotives in motion once more; pours caustic solutions into streams; and fills the skies with acrid fumes.

As Trinder shows, one of the most impressive features of the early eighteenth-century landscape was its bustling activity. Not only in ancient Norwich could Defoe see "the inhabitants being all busie at their manufactures," but also in newer textile centers elsewhere, in ironworking, coal mining, salt making, pottery, and glassmaking districts scattered throughout the Midlands and the north. To foreign visitors in the early eighteenth century, Britain appeared to be a busy trading and manufacturing nation, but there were few sights unfamiliar to natives of the Low Countries, Sweden, Silesia, or Alsace. This was to change in the classic Industrial Revolution era. What Trinder calls "the landscape of economic growth" (1750–90) was characterized by large-scale enterprises, which, like canals and railways, had a much more profound impact on the environment. So too did pollution—a much neglected consequence of early industrialization. On the slopes of hills above the Swansea copper works, according to a visitor in the 1800s, there was not "a blade of grass, a green bush, nor any form of vegetation," only "volumes of smoke, thick and pestilential."

For Trinder, "The Heroic Age" (1790–1810), when industry became increasingly urbanized, led to the "Age of the Engineer," and by 1850 Victorian genius had stamped its mark on the landscape. Manchester, as Trinder says, was a town where stock images of the industrial landscape were formed. Yet how typical was the Manchester so familiar to us from the pages of Engels? It certainly differed radically from Birmingham and its landscape, as well as its social and economic structures. Between 1850 and 1890 the industrial landscape reflected the contrasts between a nation dynamic enough to regard itself as the workshop of the world, yet containing substantial pockets of poverty and squalor—like pollution, the by-products of dramatic economic growth.

In charting the changing English landscape since the 1700s Trinder provides a refreshing view of the processes of economic growth and industrial transformation. He shows convincingly, and with few hints of antiquarianism, that the apparently well-trodden paths of research on the origins and dynamics of the Industrial Revolution would repay further exploration. This volume challenges a lot of assumptions and goes a long way toward convincing

this reviewer that if some practitioners abandoned (even temporarily) their pocket calculators for their boots and looked at what *actually* happened on the ground, the disciplines of economic and social history might well be less constrained in their approach and would be in a healthier state.

IAN DONNACHIE

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MARK NEUMAN. *The Speenhamland County: Poverty and the Poor Laws in Berkshire, 1782–1834*. (Modern British History.) New York: Garland. 1982. Pp. iv, 263. \$33.00.

In *The Speenhamland County* Mark Neuman manages to bridge the gulf between national and local poor law studies, while avoiding the misleading generalizations of the former and the amateur parochialism of the latter. With the skills and critical approaches of the professional historian, he is able to examine in detail the development of poor relief policies in an important and representative southern agricultural county over the half-century before the New Poor Law of 1834.

The author effectively and sensitively reveals the plight of the Berkshire poor squeezed by dearth, mechanization, and enclosure. On the last point, he makes a strong case for the negative impact of the enclosure movement when compared to the more benign interpretations of Chambers and Mingay. He has a particularly good chapter on the severe problem of vagrancy that beset the county, traversed as it was by the Bristol-to-London road with its incessant Irish trampers. And, of course, the complex development of the Speenhamland Plan itself, about which Neuman has published previously, is treated deftly.

Yet there are some problems with this study, and they begin in the preface. There Neuman chides fellow poor-law scholars for neglecting the "theoretical models thick on the ground which they might appropriate, wholly or in part, for themselves" (p. iii). Brandishing concepts like "social police" and "paternalist reciprocity," he urges the application of the theories of E. P. Thompson, Donajkowski, and others. After such a provocative start, it is perplexing, to say the least, to find the rest of the book devoid of theoretical constructs of any kind. The reason for this is that Neuman was not disposed to undertake a major revision of his 1967 dissertation. The results are to be seen not only in his failure to follow his own advice in regard to social theory but, more seriously, in his neglect of most of the major poor-law studies of the past fifteen years.

Since it is one of the author's central concerns to study the Berkshire justices and their relationship with parish overseers and paupers, he might well

have profited from considering recent scholarship. For example, he begins his study in 1782 because of Gilbert's act of that year but is unable to offer any convincing reason for its significance beyond its prohibition of using the workhouse for able-bodied paupers. Had he appreciated the significance of this statute as being the first systematic attempt by the gentry to acquire direct control over poor-law administration, he would have been able to link it to what he shows was a growing estrangement between parish overseers and justices. Similarly, his treatment of the Select Vestries Acts of 1818 and 1819 founders because he neglects to consider their crucial shift of power to the gentry in certain parishes through a plural voting system. Related to this is his failure to consider the tensions and very different poor-rate burdens between "open" and "closed" (gentry-dominated) parishes.

With some updating of a few such critical points, a useful study might have been transformed into an outstanding one.

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RICHARD SHANNON. *Gladstone*. Volume 1, 1809–1865. London: Hamish Hamilton. 1982. Pp. xvii, 580. £18.00.

Richard Shannon's book is a milestone in Gladstonian studies. Of the many biographies that have appeared since John Morley's classic three-volume *Life of Gladstone* was published eighty years ago, this is the first to match it in size and thoroughness. There are, as one would expect, profound differences in their approach. Morley was concerned with tracing the rise of the great party leader whose liberalism slowly broadened, in the approved Victorian manner, from precedent to precedent. It was a clever, smooth, well-rounded piece of scholarship. Shannon is more critical and more objective. He is skeptical where Morley was reverent, frank where Morley was reticent, and interested as much in the psychology of the man as in the career of the statesman. The contrast between the two biographies owes little or nothing to the discovery of new sources. Both authors had access to much the same mass of archival material. Morley did have to observe certain restrictions imposed by the Gladstone family, although it is unlikely that these were much greater than those dictated by the conventions of the age and his own sense of historical propriety. Shannon had his task lightened by the professional arrangement of the Gladstone papers when they were deposited in the British Museum and by the recent publication of the Gladstone *Diaries*. For both men, however, the real difficulty was the sheer bulk of the sources available to them. No other British

statesman has bequeathed such a mountain of paper—letters, journals, speeches, books, and pamphlets—to posterity. The *Diaries* alone, kept up for some seventy years from the age of sixteen onward, run to forty-one volumes. Gladstone poses many problems for historians, but lack of information is not one of them.

In a sense there is too much information. The *Diaries*, in particular, constitute a peculiar hazard for biographers. Shannon has made constant use of them to depict Gladstone's daily activities, thoughts, and interests. Pages of his book read like the stream of consciousness once popular with psychologists and novelists. It is a literary device that soon exhausts its value. When away from these close-packed descriptions of Gladstone's diurnal and multifarious occupations, however, Shannon writes with skill and perception. His discussions, for example, of Gladstone's relations with Disraeli and Palmerston, or his crusade over the Neapolitan prisons, are superbly done. If his general interpretations of Gladstone's motives are heavily influenced by psychological concepts, they are nonetheless penetrating and persuasive. In all, this is an outstanding work of biography: well written, scholarly, and shrewd. One is left with admiration for the ability that Shannon brought to this immense task but also, perhaps, with a tinge of regret that it was not done in fewer words, for this is a book that makes large demands on the reader.

What it does, in effect, is to go behind the respectable image created by Morley to the living Gladstone known to his contemporaries. Perhaps what emerges most clearly is Gladstone's mental and emotional fluidity or (some would say) instability. Paradoxical, even blasphemous, as it may strike those brought up in the orthodox liberal tradition, Gladstone was not a man of principle. He was endowed with titanic energy and deep emotions. All his life he was impelled by forces, often conflicting and not always commendable, that he could dimly recognize in himself but rarely admit to others. As a politician he had a dangerous facility for taking up causes with the passionate conviction that it was divinely ordained that he should do so. As this book illustrates, an absence of settled conviction was something that the more observant critics discerned at the time. Lord Aberdeen said of him that there was no "perspective in his views" (p. 239). Lord Acton wrote that he was remarkable for "the highest moral integrity and rectitude and for the utmost intellectual duplicity" (p. 411). He was, indeed, not even a genuine democrat or populist, although he always kept himself free to urge popular issues if they suited his purpose. Shannon's analysis of this explosive mixture in his first volume leaves us with sharpened anticipation for the next.

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DAVID OWEN. *The Government of Victorian London, 1855–1889: The Metropolitan Board of Works, the Vestries, and the City Corporation*. Edited by ROY MCLEOD. Cambridge: Belknap Press of Harvard University Press. 1982. Pp. xv, 466. \$25.00.

KEN YOUNG and PATRICIA L. GARSIDE. *Metropolitan London: Politics and Urban Change, 1837–1981*. New York: Holmes and Meier. 1982. Pp. xiv, 401. \$55.00.

As early as 1854 London was officially described as "a province covered with houses." This hyperbole foreshadowed over a century of conflict between the unitary and the dualistic concepts of metropolitan government. The latter system has prevailed in practice, however, so that the dispute has been largely an academic one, with a centralist *dirigisme* advocated by a school of rationalistic, socialistic intellectuals running from H. G. Wells through William A. Robson to Peter Hall, and resisted by entrenched, localist interests in the City of London and the richer suburbs, backed by the Conservative party. A full historical assessment of this long struggle has hitherto been lacking, mainly because the size and complexity of London have discouraged research. The nearly simultaneous appearance of two weighty studies, one by David Owen and the other by Ken Young and Patricia L. Garside, reflects the enterprise of what amounts to an Anglo-American team, largely inspired by the late H. J. Dyos.

Back in 1974, Dyos commissioned Young to write a history of metropolitan government, at which time he was also planning to make a key contribution to Roy McLeod's selfless effort to publish Owen's posthumous manuscript. Recognizing the dearth of work on Victorian London and suspicious of the bad press that soiled the memory of the Metropolitan Board of Works (1855–89), Owen had set out to provide a definitive history of the Board and of a sample of the district administrations that worked with it under the Metropolis Management Act of 1855.

Whereas his study of the Board was virtually complete at his death in 1968, much remained to be done on the local administrations. McLeod called on Dyos and Francis Sheppard to help him. After Dyos died in 1978, McLeod brought in two close friends of Dyos, Donald Olsen and David Reeder, to write an introduction and a bibliographical essay respectively, while the indefatigable Sheppard wrote on the City of London and a survey piece on the mid-century crisis of London's government. Meanwhile, curiously enough, Young met Garside at the 1978 AHA meeting in San Francisco, where he had been invited to act as commentator on Garside's panel at the suggestion of . . . H. J. Dyos!

That New York followed London in the two-tier system will make these studies particularly interesting to an American readership. Significantly, Owen, the professional historian, seeks to understand rather than to pass judgment; John Clive tells us in his foreword that "Owen's principal aim remained that of trying to see the period from the inside" (p. xiii). This almost Rankean ambition is fully reflected both in Owen's own texts and in the supplementary work of Olsen and Sheppard. Thanks to their combined efforts, we now have a sympathetic, full-length study of the Metropolitan Board of Works and some useful material on the City of London and four representative vestries. This is, for the most part, traditional political and administrative history, elegantly written, with Olsen in particular echoing Owen's delightfully light touch. Curiously, however, the revival of narrative history hailed by Lawrence Stone is also reflected in the Young and Garside volume, even though Young is primarily a political scientist and Garside a student of urban planning. They allow the complex London story to point to one major conclusion (foreshadowed, incidentally, by Olsen [p. 2]): that administrative solutions have always tended to be one step behind the growth of the metropolis, permitting the reformers to win the last war but never to prevent the next one. But Young and Garside also maintain that London's growing size and diversity made a federal structure appropriate from as early as the middle of the nineteenth century. Their work thus lacks the cutting edge that readers brought up on reams of London polemic have come to expect, while incorporating a somewhat uneasy transition between Young's largely institutional approach to the pre-1914 period and Garside's post-1930 LCC planning history, based largely on her doctoral thesis. It is perfectly clear that the federal structure persistently prevented a redistribution of personal income from rich to poor boroughs, while the persistence—until it was too late to matter—of county boundaries defined as early as 1847 allowed the growth of American-style independent suburbs on a scale unparalleled elsewhere in Britain, with the predictable political results. Garside detects a fascinating association of isolationism and socialist control within the LCC from the 1930s, but her emphasis on planning (which, in practice, does not extend greatly beyond housing) largely excludes other aspects of local administration, of which education is arguably the most important. Consequently, we still await a full evaluation of the *performance* of the two-tier structure, and a full analysis of the London *economy* is even further away. Owen, Young, Garside, and their associates have, however, done more than enough to reopen the debate on London government, ironically at a time when, as Young and Garside proclaim, "London displays all the charac-

teristics of the post-mature metropolis in decline" (p. 338).

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GEORGE K. BEHLMER. *Child Abuse and Moral Reform in England, 1870–1908*. Stanford: Stanford University Press. 1982. Pp. viii, 320. \$30.00.

The last generation has witnessed an explosion of scholarly interest in the history of women in Victorian society. It is curious that this admirable development has not been accompanied by similarly extensive concern for the history of children. This relative neglect by no means reflects lack of contemporary interest in the subject. No fewer than seventy-nine statutes were enacted on the subject of child welfare and education between 1870 and 1908, and these were the fruit of thousands of books and articles and countless letters and speeches urging the necessity for legislative interference.

Social historians thus have every reason to be grateful for the appearance of George K. Behlmer's careful and scholarly book. As in so many areas of social life, Victorian state intervention to protect children was partial and tardy. Propaganda from humanitarian reformers, doctors, and crusading journalists, like W. T. Stead of the *Pall Mall Gazette*, had grown impressively from the early 1860s. Damning litanies of infanticide by "baby farmers" were produced. Extensive circumstantial evidence of young children being murdered for the insurance money was compiled, in addition to almost routine accounts of dreadful beatings and habitual neglect. Characteristically, middle-class investigators concentrated their attention on the working-class areas of the larger cities. Not until 1889 was the Act for the Prevention of Cruelty to Children passed. It was, as Behlmer notes, "England's first attempt to deal comprehensively with the domestic relationship between parent and child" (p. 109). Yet this "Children's Charter" depended for the collection of evidence on which prosecutions could be brought on substantial contributions from private organizations, rather than from the central government.

Behlmer concentrates on the work of the most prominent of these organizations, the National Society for the Prevention of Cruelty to Children (NSPCC). The society's leading figure was the Reverend Benjamin Waugh, a Congregational minister. In Waugh, Behlmer uncovers a Victorian archetype: the formidably energetic, persistent, self-confident, and self-righteous social reformer. The author is particularly good on Waugh's unsuccessful bull-at-a-gate assaults on the life insurance companies in the 1890s, which threatened to undermine sympa-

thy for the NSPCC. Behlmer's account of the NSPCC and its predecessor, the London Society for the Prevention of Cruelty to Children, is clear and generally perceptive. The pity is that nearly all the society's case records were destroyed. It is thus impossible to mount a systematic comparison with the groundwork of that other great individualistic and philanthropic Victorian institution, the Charity Organization Society (COS). The richness and subtlety of Stedman Jones's treatment of the COS in *Outcast London* is beyond Behlmer here, but largely for instrumental reasons. Some may also regret that more space is not devoted to the theory and methodology of state intervention, although the penultimate chapter integrates continued pressure for reform (culminating in the sprawling Children Act of 1908) with discussion of the "national efficiency" debate.

Overall, this is a strong, empirically based study on a previously underresearched area of major importance. It deserves to be widely consulted by historians of social policy.

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IAN BRITAIN. *Fabianism and Culture: A Study in British Socialism and the Arts, c. 1884–1918*. New York: Cambridge University Press. 1982. Pp. xii, 344. \$34.50.

At first glance, Fabianism and culture must seem an unpromising topic for so large and serious a book. Were not the early Fabians notorious philistines? In fact, Ian Britain has little difficulty disposing of that myth, and he is supported by such contemporary evidence as the *Labour Leader's* report that "cultural topics are more appealing to the Fabians generally than social or political topics" (1898). Given this now unfamiliar perspective, however, what larger significance can be found in Fabian ideas on or contributions to the arts? And were there characteristically *Fabian* contributions to the arts?

Britain addresses these questions in a variety of ways. He rightly stresses that an awakening sense of art and beauty turned many Fabians toward socialism and shaped their socialist values. The agent of this transformation, in many cases, was William Morris. But what of his sharp disagreements with the Fabians and the familiar contrast between their utilitarianism and his romanticism? After exhaustive discussion, Britain concludes that the early Fabians absorbed far more of Morris's teaching than he realized, including a countercurrent of romanticism and a revulsion against commercial values that shaped their socialist outlook and their concern with the arts. This is a larger claim for Morris's influence

on the early Fabians than has previously been made, and it seems well founded.

Such excursions into intellectual history—a major strength of Britain's book—highlight the point that *ideas* about art and its social role are his real topics. Such excursions also bring Britain to a central dilemma of Fabian socialism: the conflict between the ideal of unselfish social service (involving renunciation of selfish pleasure) and the romantic ideal of self-fulfillment through free development of sensuous pleasures via art. Again, Morris is said to have shown the way out of the dilemma by renouncing forms of self-fulfillment that involved exploitation or could not be shared by all people and by pointing Fabians toward types of art that had a "direct relationship with the working lives" of the people (p. 252). (But there were also many Fabians who dissented from this view.)

Britain seeks to defend the early Fabians against three common characterizations—that they were philistines, ascetics, and elitists—but he is only entirely successful with the first. The Fabian ethos of hard-slogging political work inevitably comes across as ascetic, even if it was partially offset by the pleasures of literature and, less frequently, the arts. Britain's tendency to lump together literature and the arts forms a basic weakness in his case, for his own evidence shows that Fabians were strongly literary but were little involved with any fine arts except drama. They loved to discuss the significance and social role of the arts, however, and here (in the last section of his book) Britain finds his most effective theme: the conflict between elitism, in the form of high culture, and popular culture in Fabian ideas about the arts. His treatment of this theme is more tightly reasoned and less digressive than his other arguments, and it remains a subject of enduring importance in all democratic societies. Despite much conflict of opinion, Fabians generally followed Morris toward affirming the need for a "mass democratic base" for the art of the future (p. 253), although they were far more inclined to give such art the attributes of high culture than was Morris. In effect, they were elitists in spite of themselves, seeking to disperse their elitism into democracy.

But what of Fabian contributions to the arts? Here Britain can only point to the work of highly disparate individual Fabians, mostly writers. The distinctively Fabian achievement was on the level of ideas. In expounding those ideas, however, Britain frequently digresses, covering much familiar territory not obviously related to the arts. Thus, his book is surely longer than its subject requires. It is, nevertheless, a highly original, sophisticated, and exhaustively researched work whose value will be lasting. In the end, it is also curiously like the Fabians themselves: its tone is unmistakably ascetic, despite the occasional pleasure of anecdotes and

witticisms; its formidable scholarship marks it as elitist; but there is nothing philistine about it.

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WOLFGANG MOCK. *Imperiale Herrschaft und nationales Interesse: "Constructive Imperialism" oder Freihandel in Grossbritannien vor dem Ersten Weltkrieg.* (Veröffentlichungen des Deutschen Historischen Instituts London, number 13.) Stuttgart: Klett-Cotta, 1982. Pp. 434. DM 128.

In the past a number of German scholars have displayed a lively interest in the British empire, so it is not surprising that Wolfgang Mock should choose a segment of its history for his dissertation at the University of Düsseldorf. As a Fellow of the German Historical Institute in London, he was able to consult an impressive number of original and secondary sources and develop this present study.

After a brief description of the various leagues and societies concerned with the empire during the late 1800s and of Britain's economic and diplomatic situation at the turn of the century, Mock introduces his major topic, Constructive Imperialism, which is defined as a late phase of the so-called New Imperialism. The concept of Constructive Imperialism originated in 1901, but it was clarified and expanded by Joseph Chamberlain in his autumn campaign of 1903. More than a plan to provide safe markets for British exports and draw together the loosely knit empire, it was a comprehensive program to revitalize many aspects of British society. Mock narrates its history, embodied in the tariff reform movement of 1903–04, and then devotes separate chapters to the ideology of Constructive Imperialism and the reaction to it by the free traders. The failure of the tariff reformers is traced to the opposition of the city interest, which disliked governmental interference in economic affairs and feared the commercial dislocations imperial preference might cause; the response of the free traders, who showed that a social welfare program could be financed without resort to customs duties; and the generally lethargic reaction of the Dominions, especially India, to economic federation. The narrative continues through the series of Conservative electoral defeats down to World War I. The penultimate chapter discusses briefly the influence of the Constructive Imperialists during and after World War I through 1932 when part of their program was enacted with the support of all of the parties.

Finally, Mock analyzes the Constructive Imperialist ideology as a movement of the "New Right." Beyond the belief that most sectors of British life needed to be invigorated, certain features of Con-

structive Imperialist thinking seem particularly interesting. Some were convinced that the future belonged to large nations, and Britain, therefore, had to consolidate its empire in order to compete. Such an enlarged empire, they reasoned, would not have to depend on the balance of power in Britain's European diplomacy.

Much of the ground covered in this work has been well-trodden. Its value lies in its detail and its sharp focus on a movement that merits careful attention. It is a balanced and objective study. Written in clear, matter-of-fact German, it should be accessible to most British empire specialists and certainly will hold their interest.

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R. W. FERRIER. *The History of the British Petroleum Company*. Volume 1, *The Developing Years, 1901–1932*. New York: Cambridge University Press. 1982. Pp. xxx, 801. \$64.50.

WILLIAM STIVERS. *Supremacy and Oil: Iraq, Turkey, and the Anglo-American World Order, 1918–1930*. Ithaca: Cornell University Press. 1982. Pp. 207. \$19.95.

The argument that petroleum was the essential ingredient in American and British policies toward the Middle East from at least the closing days of World War I doubtless will always have its adherents. These two quite different books demonstrate, once again, that at least in the Middle East such policies are rarely as simple as historians might wish.

R. W. Ferrier, in his substantial history of British Petroleum (BP) shows as conclusively as possible that the crucial prewar Admiralty association with the company was "shrewd opportunism rather than deliberate policy" (p. 10)—"improvisation in response to short-term considerations [rather] than anticipation of future considerations" (p. 204). Ferrier makes no claim that his conclusion is original, but he does describe the story from the viewpoint of BP in far more detail than did Henry Longhurst in his 1959 study, *Adventure in Oil: the Story of British Petroleum*. Ferrier's massive volume—the first of a projected three—has in fact some of the awesome characteristics of definitive corporate biography, including extensive in-text and appended statistical compilations on production and finance (appendixes, notes, and index form some one hundred sixty pages in all, with the bibliography yet to come in volume 3). The work is based largely, but hardly exclusively, on BP company records, to which Ferrier had open access. A hundred or so photographs from the same archives add an interesting visual dimension.

As corporate history the work holds considerable interest, following BP from the original wildcatting

in 1901 of the William Knox D'Arcy concession into the modern world of corporate man, planning rational development of "upstream" resources and "downstream" markets. BP's early years were difficult; the 1914 contract to supply fuel to the Admiralty just saved the company, then near financial ruin, from amalgamation with rival Royal Dutch-Shell. If the Admiralty contract was salvation, however, World War I was the key to success. Heavy wartime demands from the Mesopotamian theater and the Royal Navy were coupled with the chance for the Anglo-Persian Oil Company (APOC), first developer of the concession, to buy BP itself, then a German-owned marketing firm (along with a similarly held tanker company), from the Public Trustee for Enemy Property at a good price. APOC, transformed into BP, may have lacked the one dominant Rockefeller or Mellon to shape its character, but its first and (throughout this volume) principal policy maker, "Champagne Charlie" Greenway, did much to accomplish his dream of developing a fully integrated company capable of moving its product from well to ultimate consumer.

The postwar years, however, were not all clear sailing. In addition to the general slump of the early 1920s, two problems in particular vexed the company. First, British government involvement in the company's ownership—a condition of Admiralty money—now handicapped its exploration efforts around the world, for general xenophobic distrust was multiplied manyfold in the case of a company partially owned by the British government. Ironically, at the same time, critics at home opposed government participation in a company that was engaged in such competitive efforts, in part because of potential diplomatic complications. Second, relations with Persia were never smooth. Persia was to get 16 percent of the profits, but the definition of "profits" was never fully agreed on. Suspicion, Ferrier makes clear, was ever-present concerning the country's most important industrial enterprise, which was remote from the capital and run by foreigners in the interest, it appeared, of a foreign government—and all in an increasingly nationalist atmosphere. The company, whether under Greenway or his successor, Sir John Cadman, always mistakenly believed that Persia could be persuaded with badly needed hard cash, while Persian negotiators dreamed rather of an impossibly golden future. The end result was cancellation of the concession by Riza Shah in 1932, a fitting point at which to close this initial volume.

At first glance it seems unfair to compare William Stivers's slim book to Ferrier's mighty series. *Supremacy and Oil* is clearly ambitious, however, in its attempt to describe both American and British policy toward a key area in a key period, and this not merely from the standpoint of great-power relationships in general but, as the title implies, from that of

petroleum as well. The book might also appear redundant in a sense, for this same ground has been worked by a legion of scholars—John De Novo, Laurence Evans, James Gidney, Marian Kent, Aaron Klieman, Jukka Nevakivi, and Roger Trask to name but a few.

Yet for all that, even the informed reader will find some interesting arguments here, not least because of the very ambition of the book, which enables Stivers to assess more of the forest than is visible to the woodsman in Armenian or Iraqi trees, particularly in the case of petroleum. To Stivers it was not the fear of dwindling U.S. petroleum reserves that led to an “aggressive” oil policy, as John De Novo has termed it, but rather the means—conveniently provided by the desires of American oil companies scrambling to be included in future world-wide markets—to “promote the expansion of American interests in the world economy *for whatever reason*” (p. 199). But such a goal required a stable Middle East, and stability was not to be achieved by inciting immature nationalisms, still less by renunciation of prewar capitulatory privileges. American policy, Stivers argues, was throughout the period generally supportive of British domination, and even the mandate system “was as much a mechanism to update the regime of capitulations as it was a means to fulfill the promise of independent national development” (pp. 69–70).

It is an interesting viewpoint, taken overall, and deserving of study. Unfortunately, considerations of American petroleum needs, in some ways the heart of the book, are relegated to an appendix (pp. 194–99), which might usefully be read before the body of the text. Similarly, the book is less successful in its considerations of British evaluations than of the American. Indeed, there were occasional dreams in London of persuading the U.S. to share the burdens as well as the benefits—particularly in Armenia, where the benefits were nearly invisible—but more often there was distrust, not only of American words, so often garbed in Wilsonian finery, but also of American actions that seemed to contradict those words. That both states may have been working toward the same overall end result may be the judgment of hindsight: at the time, British officials were only too eager to suspect American behavior, whether of admirals Bristol and Chester in Turkey or of American oil men in Iraq. Stivers by no means ignores this fact, but he is inclined to treat as mere irritants suspicions that, it can be argued, were a basic policy feature. Finally, the subtitle is misleading: although the Anglo-Iraqi Treaty of 1930 is given brief consideration, the work discusses events in detail only through the Lausanne Conference of 1923.

Both books are worth study, particularly by those concerned with Middle Eastern oil and its problems.

Ferrier obviously has more to say about oleaginous entrepreneurship in the early years of the century, and he tells the story well; his is a careful and scholarly work that promises to be the standard on the subject. The nonspecialist reader may learn more than desired of distillates, viscosity, and psi flow rates, but without such details the company's basic concerns in the production and marketing of oil would be unintelligible, nor would it be clear how APOC-BP not only survived but also, by the end of this volume, possessed the best-managed fields in the world. Stivers's work, however, is the more provocative, although it will no doubt annoy those with different views of Middle Eastern diplomacy and the role therein of petroleum.

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J. A. CROSS. *Lord Swinton*. Oxford: Clarendon Press of Oxford University Press. 1982. Pp. vii, 338. \$45.00.

Philip Cunliffe-Lister, Earl Swinton (born Philip Lloyd-Greame) was bluff, conventional, patriotic, and rich, the quintessential Tory. Less typically, he was also hardworking and intelligent. This short biography by J. A. Cross is basically the story of a man who for a few years in the mid-1930s was at the center of British preparations for World War II and who, although certainly part of the Baldwin-Simon-Hoare-Chamberlain axis, albeit at the second rank, fought to improve and expand the Royal Air Force, thus building the narrow margin that won the battle for the skies in August and September of 1940. It should be read in tandem with Martin Gilbert's fifth volume of *Winston Churchill*. The surprising thing is not how little was done during the period, but how much, given the environment of Byzantine intrigue that informed the last Baldwin administration, which evidently suited the prime minister but certainly retarded decision making.

To one who has long deplored the absence of serious scholarship about all but the major political figures of the post-1918 world, while being submerged in graduate student essays about the tortured conscience of the labor movement, this book is refreshing. Yet, except for the data on British military preparations, Cross's study is taut, tiresome, and often unclear. Who, for example, is Ernle Chatfield? That he was the most visible and political (not the best) first sea lord between Beatty and Mountbatten the reader is not told. What is the important difference between the Fleet Air Arm and the Royal Naval Air Service? Why was Tom Inskip so important? Indeed who, or what, was he?

This aggravating evasiveness continues throughout the volume. Too often the author gives his

readers not what they want to know but what he happened to find. For two years after Churchill came to power Swinton was head of the Security Executive and supervised all domestic intelligence and security operations. Here was an opportunity to add to the growing mass of information on undercover work during the war. Instead the largest single discussion of Swinton's activities between 1939 and 1945 is devoted to a civil aviation conference in Chicago in 1944, which, in the end, failed.

In summary, this is a useful book about an important, little-remembered man. There should be more of this genre, but with, one must ask, more explanations, delineation of character, and attention to wider themes. The average lay reader of history, whom the profession ignores at its peril, seeking to learn something of his own time, will be disappointed.

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PAUL BEW and HENRY PATTERSON. *Seán Lemass and the Making of Modern Ireland, 1945–66*. Dublin: Gill and Macmillan. 1982. Pp. 224. £15.00.

This is an interesting account of a very important transition in Irish history; it is also, in some ways, a perplexing one. Paul Bew and Henry Patterson, who teach history and political science respectively in Northern Ireland, trace the history of the shift in Irish policy from economic nationalism in the 1930s and 1940s to economic liberalization in the 1960s, from old-fashioned agrarian radicalism and industrial tariff protection to reliance on foreign capital and free trade to spur economic development. In defining the attitudes of political parties and economic pressure groups on these issues, the authors show not only the differences between traditionalists and innovators, but also the divergent approaches toward stimulating economic growth that existed among the latter. While acknowledging the contributions of others to the new departure, Bew and Patterson contend that Seán Lemass played the key role in the pivotal economic policy decisions of the late 1950s and early 1960s and that political as much as economic considerations determined his course of action as prime minister (1959–66). Lemass's expansion of public investment and involvement of trade unions in the government's program laid the basis for welfare capitalism in the Irish Republic and preserved Fianna Fail's political dominance in the process. Ironically, the party founded by the austere and parochial Eamon de Valera remained the "national party" by adopting a program of rapid modernization in place of its traditional ideal of small

farms, locally controlled industries, and "frugal comfort."

Unquestionably, Lemass played a vital role in this crucial change. The authors' arguments are plausible and often persuasive, but they would have carried more weight if supported by detailed analysis of voting patterns and the political activities of economic pressure groups. Likewise, while the authors make good use of published sources for the post-1950 period, their study would have benefited considerably from access to private papers and unpublished official records. Furthermore, although Lemass is the book's protagonist, he never really comes to life in its pages but remains an impersonal and remote figure, whose ideas and actions are sometimes hard to follow in the mass of debates, proposals, reports, and commentary that makes up the narrative. Finally, in a work that proclaims that Lemass's policies marked a watershed in Ireland's economic development, one expects some kind of reasoned assessment of their impact in the years following Lemass's retirement in 1966. This brief study is a useful introduction to its subject, but its usefulness is limited by its restricted scope and sources.

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ROBERT M. JENNINGS and ANDREW P. TROUT. *The Tontine: From the Reign of Louis XIV to the French Revolutionary Era*. (S. S. Huebner Foundation Monograph Series, number 12.) Philadelphia: Wharton School, University of Pennsylvania; distributed by Richard D. Irwin, Homewood, Ill. 1982. Pp. xi, 91. \$14.95.

The tontine was named after Lorenzo Tonti (ca. 1620–84), an Italian adventurer who came to Paris under the wing of a patron, another Italian adventurer, Giulio Mazarini (1602–61), chief adviser to the regent queen mother from 1643 to 1661. On Mazarini's advice, the crown approved Tonti's project in 1652 and thereafter tried it from time to time in the seventeenth and eighteenth centuries. Robert M. Jennings and Andrew P. Trout have summarized the history of the official French tontines of 1689, 1696, 1709, 1714, 1733, 1734, 1743 (two), 1744, 1745, and 1759 and of some Dutch, English, German, American, and private French projects as well. The weakest part of their study is their account of the late eighteenth-century tontines. They seem to know nothing of such fundamental studies as Jean Bouchary, *Les compagnies financières à Paris à la fin du XVIII^e siècle*, volume 1, (1940), with additions in Bouchary's later work, *Les manières d'argent à Paris à la fin du XVIII^e siècle* (1943). Their pamphlet, for such it is, will serve as a

useful introduction for students of business history, but for a historian it lacks breadth and depth.

The French historian sees the tontine as one among many expensive fund-raising devices adopted by French kings out of sheer political weakness. Unable to devise and implement a funded public debt, a central bank, or a modern treasury, such as the British Parliament adopted in the half-century after the Revolution of 1688, the Bourbons resorted to short-term, emergency financing until the French Revolution at last accomplished what had begun in England a century earlier. Jennings and Trout list P. G. M. Dickson, *The Financial Revolution in England* (1967), but they seem to have missed its implications for the political and administrative history in which the tontine belongs. Likewise, they have no room to discuss those groups of financial speculators who bought the tontines, a deep and difficult subject of the greatest consequence in French history. Their research in French archives was hasty and superficial. Their pamphlet is partly redeemed, however, by their attention to the technical side of the tontine.

Jennings and Trout fearlessly discuss the actuarial basis of the tontine, which was an annuity yielding a fixed income to a limited group of investors, who each received more and more over the years as the shares of the dead were divided among the survivors. Historians who are timorous in technical matters may be interested in the work of Jennings and Trout, whose pages would add spice to the bland political treatment of Bourbon revenue schemes in the standard work by Marcel Marion, *Histoire financière de la France*, volume 1 (1927). The tables, financial judgments, and discussions of how each tontine worked are serious enough to warrant the attention of any student of Bourbon finances.

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LAURA S. STRUMINGHER, *What Were Little Girls and Boys Made of? Primary Education in Rural France, 1830–1880*. (SUNY Series in European Social History.) Albany: State University of New York Press, 1983. Pp. viii, 209. Cloth \$30.50, paper \$9.95.

Primary education as a means of instilling middle-class values among the urban and rural poor in nineteenth-century France has recently been receiving historians' attention. In this volume Laura S. Struminger presents primary education as a vehicle for bringing modern values to the countryside and examines the effect of the education on its recipients. The book has three parts: an introductory discussion of primary education, an extensive translation of two texts, and a brief conclusion.

Part 1 offers a general description of the poverty of rural schools, the economic plight of teachers, the lack of adequate materials, and parents' opposition to sending their children to the public primary schools. Struminger argues that rural families opposed church schools less than they did public ones, and she describes the church-state interrelationship in primary education. The introductory chapters provide an interesting background for the main portion of the book.

The central body of the book consists of an edited translation of two texts for French school children by Zulma Carraud, *La petite Jeanne ou le devoir* and *Maurice ou le travail*. These texts, first published in 1852 and 1853, were adopted in over twenty departments and saw many reprintings. Struminger presents long excerpts to illustrate the use of these books for gender role socialization; they constitute "a case study of the attempt of the bourgeoisie to inculcate values" (p. 3). Carraud's protagonist Jeanne is clean, hard-working, domestic, obedient, dutiful, cheerful, educated, maternal, and devoted to others. Maurice works hard, perseveres, is honest, learns a trade, is loyal and devoted to his master, supports his family, accepts progress, and plans for the future. Both Jeanne and Maurice prefer hometown rural life to that in the city. Their virtues lead to success. Struminger concludes that differentiation into separate spheres for boys and girls began in primary school.

Although Carraud's books received wide circulation, Struminger notes that many texts, including those of Rocherolles, Lorrain, Kergomard, and Fabre, were used in far more departments than the books of Carraud, but she does not discuss them or describe their differences. Analysis of the political and socioeconomic characteristics of the departments that adopted Carraud's books might have led to new understanding of the relationship between regional differences and modernization.

In part 3 Struminger attempts to show the impact of education on French children. She draws from four hundred children's notebooks in her discussion of the influence of educational materials and quotes from a few. It is difficult, however, to judge how representative the notebooks are with respect to department, chronology, and age and sex of the child. Struminger concludes that, while "some accepted new ideas, and others rejected them" (p. 143), in "hope of a better life . . . rural boys and girls gradually adopted the roles of Maurice and Jeanne" (p. 152). Attitudes toward children and the family were changing considerably by the 1880s. Stronger linkage of the values taught by the textbooks to attitudinal changes would have been most welcome.

The illustrations are interesting, but descriptive captions are incomplete. Data in tables and appen-

dixes are important, but their value would be increased with more interpretation and discussion and with full source reference. For example, an analysis of the data might elucidate the problems, financial support, and socioeconomic and gender differences between the public and private school populations in the different departments. This book is valuable for the translation and reproduction of much of the two important texts by Carraud. It falls short, however, of fulfilling the need for a thorough analysis of primary education in rural France.

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WILLIAM H. SCHNEIDER. *An Empire for the Masses: The French Popular Image of Africa, 1870–1900*. (Contributions in Comparative Colonial Studies, number 11.) Westport, Conn.: Greenwood Press. 1982. Pp. xxi, 222. \$29.95.

Who were the French imperialists? Historians have provided a variety of answers to this question. Their identification of empire-builders has focused on various elites or interest groups: the merchants of Lyon and Bordeaux, the “colonial party” in Paris, the military proconsuls on the frontiers of the empire, or the geographic societies and their explorers. Generally, historians of the French empire have assumed little popular interest in empire-building among the mass of the French population, including the electorate.

Thanks to William H. Schneider’s valuable study, we now have a new dimension to the composite portrait of those interested in the French empire at the end of the nineteenth century. The empire of the elites was also the empire of the masses, and to the “official mind” of French imperialism a public mind has been added. Schneider identifies “the masses” as readers of the expanding penny press, notably *Le Petit Journal*, and of the illustrated press, exemplified by *Le Petit Parisien* and *L’Illustration*. He also makes effective use of the ethnographic exhibits and, eventually, shows in Paris at the Jardin d’Acclimation to trace the shifting image of Africa and the Africans in the popular mind. Schneider documents the growing readership and rising attendance at the displays. Nevertheless, an assumption must be made, despite the use of information theory and statistical analysis, that these new readers and avid witnesses were “the literate workers, lower middle-classes and peasants” of France. The argument is reasonable and persuasive.

The popular empire for the masses did not initiate the drive for empire. Schneider recognizes that government policy preceded reporting on the events of imperial conquest (p. 44). Although popu-

lar imperialism did not inspire expansion, Schneider can argue that at least there was no popular resistance or hostility to imperialism. He once more lays to rest the claim that Jules Ferry was swept from office on a wave of anti-imperialism. The editors promoted imperialism, and the readers went along with enthusiasm.

The case for empire included an economic rationale. Here Schneider detects a social imperialism with empire promoted as a means to recovery from the post-1882 recession. Yet the “Africa of exploitation” proved disappointing in both benefit and as a prescription for empire. This image of overseas economic opportunity shared place with Schneider’s “Africa of conquest,” which was also used to justify the costs of expeditions to overcome African resistance. A new official image, tailored to popular expectation, of an empire of exploitation emerged at the turn of the century with expositions in Paris and Marseille. Though these two parallel images of empire coexisted, Schneider argues that the economic concern remained central.

Among the book’s strengths are carefully selected illustrations that reveal directly the nature of the African image presented to the French masses. As the immediacy of imperial conquest became known in Paris, the result was a late nineteenth-century version of a “living room war” with the images properly shaped to reinforce French national pride. But the images presented were selective in another way. The Dahomey campaign appears to have been blown out of proportion. Still, Schneider’s chapter on images and his discussion of the ethnographic exhibits constitute original and important contributions to our understanding of the popular empire and its image.

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CHRISTIAN BAECHLER. *Le parti catholique alsacien, 1890–1939: Du Reichsland à la République jacobine*. (Association des Publications près les Universités de Strasbourg.) Paris: Ophrys. 1982. Pp. xxi, 764. fr. 240.

One of the merits of this massive study is that it traces the fortunes of a major regional political party through the period between 1870 and 1918, when Alsace-Lorraine was annexed to the German empire, and then through the interwar period, when the provinces were reunited with France. By 1918 the Catholic party had become one of the three largest in the region, and by 1939 it was the largest. Its broad electoral appeal indicates that to a considerable extent it reflected the concerns of the

inhabitants of the controversial provinces during two difficult periods of adjustment. Although organized to protect Catholic interests, the party soon became involved in the defense of regional autonomy against the demands of the imperial government in Berlin, which was not sympathetic to a loose federative arrangement. To the extent that the Catholic party found it necessary to protect religious rights in the face of national anticlerical policies, it cooperated with the German Center party. But its leaders realized that the party's influence in Alsace-Lorraine would decline if it became too closely identified with a national party. Anticlericalism as a political issue affecting the provinces became even more acute after reunification with France. The dilemma facing Catholic party leaders at that time was whether Catholic interests were best served by uniting with other French conservative parties or by staunchly reasserting provincial rights in the face of the centralizing policies of the government in Paris.

The defense of regional interests against the national requirements first of Germany and then of France is not the only significant theme in this useful study. Christian Baechler also analyzes the impact of liberal Catholicism and Christian socialism on the Catholic party of Alsace. Liberal Catholics urged the party to unite with other liberal groups in support of religious rights and bourgeois interests that appeared to be jeopardized to an increasing extent by the rising tide of socialism in the provinces. But the party was more influenced by Christian socialist principles that sought to reconcile social interests and called for the intervention of the state, if necessary, to alleviate the grievances of the working classes. The Christian socialist influence enabled the party to increase its grass-roots support particularly in rural areas and small towns. Its popular appeal in turn enabled the party to become a democratizing influence in the region during the period of annexation to Germany, thereby facilitating its reunion with democratic France in 1918. The party's continued grass-roots support after 1918 resulted from its awareness of the adverse effects of economic dislocation during the interwar period and from its support of regional interests in the face of renewed tensions between France and Germany after 1933. The Catholic party's growing preoccupation with social and regional problems during the 1930s made it seem less sectarian and thereby more attractive to non-Catholic voters.

This exhaustive study, based on French and German archival materials, deserves the attention of anyone interested in regional and Catholic politics in the twentieth century as well as the process by which an elitist political formation became a party with mass political appeal.

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JEAN-NOËL JEANNENEY and MONIQUE SAUVAGE, editors. *Télévision, nouvelle mémoire: Les magazines de grand reportage, 1959-1968*. Paris: Seuil or Institut National de l'Audiovisuel. 1982. Pp. 250. 75 fr.

This book has an explicit underlying purpose—to insist that the time has come to construct a scientific history of French television. The editors, Jean-Noël Jeanneney and Monique Sauvage, believe that historians have neglected television, just as they had earlier neglected cinema. They argue convincingly that television is too significant a medium to be ignored and suggest a general line of research that they feel should be undertaken.

The product of a seminar conducted by the editors in collaboration with the Institut d'Études Politiques de Paris and the Institut National de l'Audiovisuel, the work attempts to develop a method for exploring the history of television, while at the same time examining a particular concrete subject, the news magazines of the 1960s, *Cinq colonnes à la une* and *Zoom*. The editors are the first to admit that the result is not a finished product.

The study suggests that these programs were based on the realization that television could provide an original form of journalism. They operated within severe political constrictions, a fact of which anyone who watched French television during the Gaullist era must be aware. The staffs, to be sure, took advantage of whatever room for innovation existed. In particular, *Zoom* under Harris and Sédouy showed itself irreverent and *prégauchiste*, with an irreverence that was often turned against the official left opposition as well (which must have made the magazine more palatable to the government). But the purge of the ORTF resulting from the events of May 1968 ended the experiment in free expression; the avant-garde was purged. Interestingly enough, some of its leading figures have now returned to positions of authority. It will be revealing to follow the evolution of the ORTF under Mitterand.

The limitations of this book stem from its very nature. It is a collective work; its contributions are unequal in both style and content. Most of the articles are informative. At least one of them, supposedly devoted to the portrayal of strikes, loses sight of its subject and wanders off into a boring and superfluous presentation of semiotic theory. It should have been cut or rewritten. Many themes are hinted at rather than carefully analyzed because they fall between the various essays. For example, there is no clear statement about what controls, direct or indirect, were exercised by the political authorities over program content. Moreover, the editors do not compare French news magazines with their counterparts in other countries. It is surprising that, although American programming was in ad-

vance of its French counterpart, there is no discussion about the influence or lack of influence of American television on the French. Nonetheless, the work does represent a beginning in the study of an important but neglected field. One hopes that further monographs will follow.

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JOHN EDWARDS. *Christian Córdoba: The City and Its Region in the Late Middle Ages*. (Cambridge Iberian and Latin American Studies.) New York: Cambridge University Press. 1982. Pp. xv, 240. \$47.50.

Spanish local and regional history has flourished over the last two decades, with an outpouring of monographs solidly based on extensive archival research. John Edwards's study of late medieval Córdoba concentrates on the fifteenth century and the period up to the death of Fernando in 1516. The majority of the recent works deal with the sixteenth and seventeenth centuries, and thus it is useful to have a new work on a slightly earlier period.

The nature of the fifteenth-century documentary base has determined to a certain extent the nature of the book's coverage. Demographic records, for example, are rudimentary before the early sixteenth century, and notarial records become reasonably complete only in the late fifteenth century. Most of Edwards's citations from the Cordoban archives are in fact from the reign of Fernando and Isabel. For earlier evidence, he mainly uses printed sources and secondary material.

Topically arranged, the work does an admirable job of delineating the city and its region, the old kingdom of Córdoba, an area slightly larger than the modern province. There is a clear depiction of the organization of the urban government, with discussions of its competence, its resources, its members, and its activities. Financial records allow a full treatment of local and royal taxation. The most satisfying sections deal with the economy, particularly the tensions between agriculture and grazing, and with the role of the regional nobility, who were "the only significant group in Córdoba's politics in the late Middle Ages" (p. 131). The economic and political influence of the nobility helped to ensure that the Cordoban region exported commodities, particularly wool, and failed to establish a sound food supply or to develop its local industry. Throughout the book Edwards demonstrates that Fernando and Isabel favored the nobility and ratified its members' social prestige and economic control.

In his discussion of religion, Edwards deals with the dominant Christian majority in manifestations

of both communal religion and personal faith and deeds, the latter revealed mainly through testaments. Córdoba also retained small communities of Muslims and Jews until they were converted or expelled. The *conversos* of Jewish origin were targets of Old Christian hostility, which culminated in the anti-*converso* rioting of 1473 and the establishment of the Inquisition in Córdoba in 1482. Here Edwards argues that the Inquisition was established "not only to enforce religious purity but also to curb seigniorial influence" (p. 184).

One problem is the author's subtle but persistent tendency to look forward to the problems of later Habsburg Spain as he analyzes fifteenth-century conditions and developments. This leads him in places to bleaker depictions than the documents themselves may justify.

Edwards has extracted an impressive amount of raw material from the archives, and he has used secondary studies ably. A satisfying and informative treatment of an important city and region, *Christian Córdoba* is a worthy addition to the historiography of late medieval Spain.

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JEROME R. MINTZ. *The Anarchists of Casas Viejas*. Chicago: University of Chicago Press. 1982. Pp. xvi, 336. \$20.00.

This is an ethnohistorical study of an Andalusian town and its 1933 anarchist uprising. The book has three sections of which the second is, perhaps, the most novel. It describes the social tensions during the first Republican biennium but centers on the preparations for the anarchosindicalist national revolutionary strike and its subsequent failure and on the independent decision of the Casas Viejas union to revolt on January 11. What follows is the most thorough reconstruction of this twenty-four hour insurrection and the ensuing repression, which left some eighteen people killed by the Assault Guards in violent retaliation for three Civil Guards mortally wounded in cross fire.

Jerome R. Mintz relies heavily on oral accounts by townsfolk, which, though engaging, may not be an accurate recollection of events long past. The author is generally uncritical of their intrinsic historical value or their possible subjective biases. So involved is Mintz that he overlooks wider problems, resulting in defects that often annoy more than inform those familiar with the topic: Casas Viejas becomes a decisive motive for Franco's rebellion forty-two months later, whereas the opposition between anarchists and republicans appears as "the heart of the struggle in Spain in the 1930's" (p. 9).

Such limitations also surface when examining nineteenth-century anarchist clandestine organizations, such as the Black Hand. Mintz perceives them and related evidence as manufactured "by the authorities and other interested parties" (p. 24) to suppress dissent, although repressive governments did not require much evidence to persecute popular movements. The surviving documents are a tribute to nineteenth-century militants who organized, drafting their own regulations, and who—like those of Casas Viejas years later—sometimes were self-taught and knew that literacy was a weapon for liberation. *Mutatis mutandis*, in the 1880s anarchists associated, conspired, and rebelled and were as brutally repressed as their twentieth-century descendants, without the authorities needing to forge the documents Mintz dismisses as "inconceivable" among peasants who "could not read or even write their initials forward" (p. 24).

Difficulties with Spanish and printing, historical, and other errors often mar the text: nineteenth-century disestablishments are placed either a century earlier or years later; dates for Primo's dictatorship and the spelling of Sanjurjo are systematically wrong; the protagonist town is embarrassingly miswritten on the rear map. Possibly because of difficulties with local phonetics, the author always transcribes *pegujales* as "*perjuales*" and includes his invention in the glossary! Errors of usage abound: among the most extravagant of them are the antonym of *obrero consciente* (conscious laborer) as "*inconsciente*," which, given the context, means irresponsible, and explaining "*tercio*"—the paludal fever known as *terciuna*—as "exhaustion from hard labor and lack of nourishment" (p. 52).

This book provides rich insights into life in Casas Viejas and a fascinating account of its revolt, but it falls short as a re-examination of Andalusian anarchism. Its challenge to previous historiography is successful only in that it provides the details of a long-neglected episode. Yet, historians seeking to learn more about other facts of a complex past will not be enlightened.

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EDDY VAN CAUWENBERGHE, *Het vorstelijk domein en de overheidsfinanciën in de Nederlanden, 15de en 16de eeuw: Een kwantitatieve analyse van Vlaamse en Brabantse domeinrekeningen* [The Royal Domain and Government Finances in Fifteenth- and Sixteenth-Century Netherlands: A Quantitative Analysis of Flemish and Brabantine Domain Accounts]. (Historische Uitgaven Pro Civitate, reeks in-8°,

number 61.) Brussels: Gemeentekrediet van België. 1982. Pp. 434. 950 F.

In France the domain was the major source of royal income during the thirteenth century, while tax revenues became far more important than the domain to the kings during the fourteenth century. But in the Netherlands the domain still provided most of the revenue of the Burgundian and early Habsburg dukes. Although the financial importance of the domain declined sharply in the early sixteenth century, it continued to provide an average of 15 percent of the income of the state as late as the eighteenth century. This important study by Eddy Van Cauwenberghe, an exhaustive computerized analysis of the manuscript domain accounts, primarily between 1384 and 1555, considers the domain both from an administrative and an economic standpoint.

The domain included not only land and rents but also tolls, assizes, and various incomes in the towns. Van Cauwenberghe focuses on five representative domains in Flanders and two in Brabant. He distinguishes ordinary domain revenues of the "old" type, involving fixed payments in kind or specie, from those of the "modern" type, the yields of which were flexible. Incomes of the old type were more lucrative on most domains than were the modern, which were hard to institute in a customary regime and tended to fall sharply with economic, political, or military problems. Paradoxically, although the Burgundian dukes tried where possible to promote the use of modern-type incomes, such as by converting hereditary tenements to term leaseholds, the profitability of their domains was achieved primarily through revenues whose face value did not vary. The explanation for this situation, particularly in Brabant, is that fixed rents in grain were normally indexed to the market price and often were expressed as a percentage of the harvest. Domain receivers could sell grain or even speculate on the market. Van Cauwenberghe finds that domain prices on grain were usually representative of the market value until the last third of the fifteenth century but were appreciably lower thereafter. The accounts show that leasehold rates rose until the 1430s, then declined until the late fifteenth century, and rose again in the sixteenth. They thus reflected an agricultural depression so severe that the prince generally benefited, even against his own efforts to "modernize," from having so much of his domain revenue expressed in a fixed face value.

While the domain receipts are the best source of information on the rural economy, expenditures show the impact of the prince and his bureaucracy as they attempted to integrate the very profitable domains into their financial structure. Van Cauwenberghe distinguishes three periods of do-

main policy. Between 1384 and 1467 the dukes built on the solid foundations laid by their Flemish predecessors and centered their financial structure on the domains. Philip the Good was especially important, for he tried to pay the obligations of the central government by letters or mandements redeemable on domain incomes. It was this practice, which amounted to a centralized system of disbursement, rather than unprofitability of the domains that explains the negative annual balance shown by many domain accounts during the mid-fifteenth century. Philip's role also explains the negative correlation that Van Cauwenberghe finds for the fifteenth century between periods of extraordinarily high expenditure by the central administration and pressure on the domain. Contrary to his public image, Philip the Good was a prudent manager of money who left a surplus in his treasury.

Beginning with the rule of Charles the Rash in 1467, the dukes preferred to leave a high balance at year's end in the domain accounts. Rather than using the domains to pay for his expensive adventures, Charles borrowed. His Habsburg successors made some attempt to return to the traditions of Philip the Good, but their readiness to levy taxes and the ease with which they could borrow money made them incautious. Thus from 1515 the dukes used the domain primarily as security for their loans. There is accordingly a correlation in the sixteenth century, as there had not been in the fifteenth, between high state expenditure and ducal pressure on the domain to pay those expenses. Substantial alienations from the domain had begun under Charles the Rash, and after a hiatus Charles V resumed the practice. The domain thus became much less important as a source of income for the dukes.

Although Van Cauwenberghe does not convince me of the wisdom of Philip the Good—what he sees as an attempt to integrate the domain into a central financial structure and create a floating debt may also be regarded as hand-to-mouth financing, for local officials were left unpaid and Philip issued mandements on the domains for funds that simply were not there—my only serious reservations about this book are organizational. Van Cauwenberghe at times goes into unnecessary detail about the central administration, when a more concise treatment would tighten his book and not affect its original conclusions. His organizational scheme is episodic, and he does not facilitate the reader's task by leaving so many of the tables and graphs on microfiches. But this is a major work that must be considered by any student of agricultural or administrative history in the fifteenth and sixteenth centuries.

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RICHARD REITSMA. *Centrifugal and Centripetal Forces in the Early Dutch Republic: The States of Overijssel, 1566–1600*. Amsterdam: Rodopi; distributed by Ronald Rupke, Weston, Canada. 1982. Pp. 337. \$18.00.

Richard Reitsma has produced a solid, workmanlike monograph on the provincial states of Overijssel, in the northeast Netherlands, during an important formative period of the early Dutch republic, 1566–1600. Working with difficult sources, he builds a convincing case for the particularization of the north Netherlands and the polarization that continued to plague the United Provinces into the seventeenth century. Simultaneously, Reitsma depicts the forces that held the fragile republic together.

Local history provides the direct focus for Reitsma's thesis. He does full justice to the complex interaction of various Overijssel constituencies: the cities; the nobility; local officials, such as magistrates; and provincial officials, such as legal functionaries.

One problem for Reitsma's English-speaking readers is, however, that far too many highly technical Dutch words remain untranslated in his text. Reitsma's need to impress his Dutch scholarly audience with his mastery of the language and literature is completely understandable. But he also needs to ensure that his finished product is accessible to the English-speaking reader, since that was the language he chose for his study. One solution might have been to put the Dutch words in parentheses, as Reitsma does in fact do on a few occasions. To define *verponding* as "land tax" and *krijchoverst* as "commander" renders the terms intelligible; but words such as *gereformeerd*, *drost*, and *heerlijkheid* (to cite only three examples from the first fifteen pages) need to be dealt with similarly. Reitsma translates with sensitivity and enormous care. Unfortunately, he fails to maintain this principle throughout the text.

This linguistic inconsistency reflects a larger problem. Whom, exactly, was Reitsma writing for? If he is writing for an English-speaking audience not steeped in the intricacies of the history of the Low Countries, he should have elaborated more on events such as "Rennenberg's defection" of 1580. This episode provided an ideal example for Reitsma's linkage between local and national events; a lengthier discussion of it would have been welcome.

There is one final issue in regard to this book. Reitsma is so concerned with the trees that he has some difficulty seeing the forest. His work contains a wealth of fascinating detail; sometimes, however, the analytical thread becomes submerged in it. Based on the title, Reitsma aims to explain the integration of political forces within individual provinces of the Dutch republic with the larger world beyond. The idea is worthwhile, but Reitsma was

not able to realize it comprehensively in this volume.

These questions are not meant to detract from Reitsma's achievement. Those working in areas related to the early modern Netherlands or to the history of political institutions such as estates will recognize it as a wholly capable monograph, successful as far as it goes, and read it with profit.

SHERRIN MARSHALL WYN'TJES
Mount Ida Jr. College

HANS CHR. SOLTER. *Adel og embede: Embedsfordeling og karrieremobilitet hos den dansk-norske adel, 1588–1660* [Nobility and Office: Distribution of Offices and Career Mobility among the Danish-Norwegian Nobility, 1588–1660]. (Skrifter Udgivet af det Historiske Institut ved Københavns Universitet, number 13.) Copenhagen: Danske Historiske Forening, 1982. Pp. 128. 65 KR.

One is reminded of certain mail-order hucksters who advertised a device guaranteed to kill flies. The package they sent contained two small pieces of wood and directions for their use. "Place the fly between the boards—and squeeze." Well, it was not exactly a swindle and neither is Hans Chr. Wolter's book. In both cases, however, we have a form of "overkill." Having bought the package, Wolter finds it necessary to spend at least a third of his publisher's expensive (but Carlsberg Fund-subsidized) space performing the quantitative historians' required ritual dance, capturing, describing, isolating, and immobilizing his chosen subject matter. The top twenty-five jobs in the political-administrative apparatus of the Danish-Norwegian state in the period 1588–1660 are carefully lined up opposite a carefully selected sample of 571 noble persons who could have or did fill them. Then squeeze! Text and charts describe how the selected human material flowed through a limited number of similarly selected channels into the seats of power.

Wolter does provide a fair amount of incidental instruction in Danish history, biography, and historiography as he maneuvers his subject matter for analysis. Most interesting, too, are the questions and people brushed aside as not fitting, not to be dealt with. In the substantive portion of the book (some forty pages), correlations provide a small flood of figures for such things as father-son successions in various offices and the relationships between types of education and career success. Shifts in career patterns are noted in connection with the flow of larger historical developments. To enliven the rather sterile quantifications, Wolter has the grace throughout his text to describe a number of exceptional as well as typical careers and, not least, to mention some notable women. He also adorns his book with good reproductions of portraits and

sculptures of some persons discussed. A nice little book!

One does not, however, escape the sense of "overkill." Do we not already know, "to a reasonable certainty," that in the selected period of Danish history young nobles who waited on royalty at court had a better chance to obtain big fiefs and other high office than those less well connected? Do we not know that placing a son to be raised in the household of influential relatives enhanced his chances in life? The statistics confirm this sort of thing. But the excellent Scandinavian prosopographic literature that makes Wolter's study possible also makes it, in a sense, redundant, except as an exercise, a demonstration of technique adequately mastered, and a gracious presentation of largely self-evident truths. It leaves livelier questions still buzzing around.

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TORKEL JANSSON. *Samhällsförändring och sammanslutningsformer: Det frivilliga föreningsväsendets uppkomst och spridning i Husby-Rekarne från omkring 1850 till 1930* [Transformation of Society and Forms of Organization: The Rise and Diffusion of Voluntary Associations in a Rural District, 1850–1930]. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 124.) Uppsala: University of Uppsala; distributed by Almqvist and Wiksell International, Stockholm. 1982. Pp. 317. 135 KR.

Torkel Jansson's dissertation deals with the impact of socioeconomic change on the fortunes of two voluntary associations in a small county in central Sweden at the turn of the century. The book contains four major sections: a convoluted discussion of Jansson's indebtedness to an international host of scholars; a fragmented introduction to changing conditions in Husby-Rekarne, a largely rural county situated just south of Eskilstuna; an often stupefying combination of econometric, sociological, and narrative analysis of the rise of a temperance movement (affiliated with the International Order of Good Templars) in the 1890s and a workers' commune in the 1910s; and an extensive, promising chapter on recruitment patterns by both county associations until the 1930s. In addition, Jansson provides summaries in Swedish and an inelegant English translation, statistical appendixes, and a bibliography.

If one uses Coleridge's distinction between enduring works (compounds) and lesser imitations (mixtures), Jansson's book is decidedly a mixture in which all the constituent parts remain distinct and unintegrated in the final product. For example, he

never links the background material on industrialization and urbanization in late nineteenth-century Sweden as a whole with particular developments in the county of Husby-Rekarne. Often the prose deteriorates into jargon, such as transitions from feudal to capitalistic conditions, from landed to urban loci of political power, and from communalistic to free social relations. Substantive fragmentation is aggravated by a paradoxical lack of theoretical integration. In spite of his assumption that historical-materialistic explanation exists for the rise and spread of both associations, he obscures this potential unifying element by subdividing his materialistic interpretation into three methodological orientations.

The dubious explanatory value of Jansson's mixture of substance and theory can, at times, even obscure the intriguing material he has compiled on the social composition of both voluntary associations. His study of the temperance movement is especially thorough. The temperance lodges were formed by individuals who had minimal impact on local policies during a period when drinking was not perceived as especially excessive in Husby-Rekarne. The absence of an explicit socioeconomic problem compels Jansson to use a "structural" explanation instead; the temperance lodges, with their nonstatus, voluntary membership criteria, provided a "socioideological" support system for the powerless from all social classes in the county, permitting them to re-create a sense of self-worth in the 1890s when a small group of capitalizing landowners and industrialists, who had already atomized the traditional social relations of production, also solidified their control of local politics. A modification of the franchise in 1909, however, gave the temperance lodges increased political influence. A second "structural" change occurred thereafter as lodge membership and leadership became increasingly "middling" in composition.

Just as Jansson's analysis of the temperance movement achieves some momentum, he stops abruptly to hoist the banner of the "capital logic" school over the emergence of a workers' commune (and, to a lesser degree, a Social Democratic youth club) during the World War I era. By "capital logic," I think Jansson means "pure material" explanations because he emphasizes that the Hållsta Workers' Commune was especially responsive to economic hardships encountered by local factory and agricultural workers. But this interesting conception of a trade union, loosely affiliated with Sweden's Social Democratic party and encompassing workers in capitalizing agriculture as well as industry, is also left incompletely examined. Jansson shifts his theoretical stance again, reintroducing a "structural" analysis to explain how another change in voting rights coincided with the exclusion from the commune of most

agricultural workers by an emergent "labor aristocracy" of railroad and craft workers.

Jansson's most significant contribution lies in the correlation he establishes between increasing association memberships and decreasing communal relations of production in different parts of the county. Most intensively and consistently active in the voluntary associations were individuals from the lower and lower-middle classes who either lived in the towns or on estates with few crofters and an increasing number of landless workers not housed in "estate communities." But Jansson squanders yet another marvelous opportunity to dissect the impact of capitalization on social relations by focusing more on his third methodological school—"Marxist cultural geography"—than on the meaning of the data themselves. For example, his data suggest that the adjustment to capitalization in the county was uneven and gradual, but his reductionistic approach to materialism assumes uniform and instantaneous change. Jansson's meticulously compiled evidence about a complex process of economic and social change is trivialized by theoretical jargon that will baffle everyone, with the possible exception of Jansson's coworkers in the project on *Klassamhällets funktioner: Folkörelserna* (Functions of Class Society: Popular Movements) at Uppsala University, under whose auspices this dissertation developed. While I applaud Jansson's conception that historical writing is amendable to theoretical perspectives, his book does not reflect effective historical analysis. It lacks a special quality that I liken to the growth of a mushroom, where the theoretical filament courses unobtrusively through the material soil until it eventuates in a stunning form.

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TOIVO NYGÅRD. *Suomalainen äärioikeisto maailmansotien välillä: Ideologiset juuret, järjestöllinen perusta ja toimintamuodot* [The Finnish Extreme Right between the World Wars: Ideological Roots, Organizational Basis, and Forms of Activity]. (Studia Historica Jyväskyläensia, number 25.) Jyväskylä: Jyväskylän Yliopisto. 1982. Pp. viii, 154.

Toivo Nygård's book is the latest addition to what is finally becoming a substantial literature on the extreme right in interwar Finland. He finds the ideological roots of that right wing rather farther back than most of its students, in the nineteenth-century linguistic dispute between extremist Finnish-speaking and extremist Swedish-speaking Finns, neither of whom, of course, got their way in molding Finnish society. Rather than the Finnish Civil War of 1918, whose importance he downgrades, Nygård sees the Finnish response to Russification after

1899, especially in 1905, as decisive in creating the ideology of the interwar extreme right. For him, the conservative Old Finnish party, which sought accommodation with the grand duke of Finland (the Russian emperor) is apparently insignificant, since he concentrates instead on the Young Finnish party, generally identified as the party of Finnish liberalism, and the Agrarian party, which was later appropriately rechristened the Center party. However mistaken Nygård is in identifying the intellectual parents of the interwar extreme right, his analysis of the activist opponents of Russification, who used illegal means, including violence, sheds much light on later right-wing tactics.

Nygård argues that there was no real coherence, only separate, if often overlapping, organizations, in the interwar extreme right. His list of these organizations is distinctive. He omits the Civil Guards, the continuation of the victorious White Army of 1918, because they were not, he claims, right wing. He discusses at length several minor organizations, including the Independence League, *Pro Carelia*, and the journal *Suunta*, none of which merits such emphasis. Three major organizations, the Academic Karelia Society, the Lapua Movement, and the People's Patriotic Movement, are included. The last of these organizations is perceptively treated as a party of the Finnish educated class. Nygård writes at length about the Academic Karelia Society, even though he argues that it was not a political organization. Such an argument is questionable, because that society had as its major aim the creation of a Greater Finland, which could only be achieved by Finnish acquisition of part of the Soviet Union. Nygård's account of the Lapua Movement is seriously weakened by his assumption that the organization was simply anti-Communist, and not aimed against parliamentary democracy. His focus on a few organizations means he pays insufficient attention to military officers and clergymen, as well as to unaffiliated lone wolves, especially Gustaf Mannerheim. His approach is like studying Gaullism without Charles de Gaulle.

The shortest, and best, part of Nygård's book deals with the forms of activity of the extreme right. There is an informative analysis of political socialization of secondary and university students by the Academic Karelia Society. The use of demonstrations and of mass confessions of political crimes, to tie up the judicial system, is illuminated. After an incisive description of the use of violent tactics, including assassination, beatings, kidnapping, and an attempted coup d'état, by the extreme right, Nygård astonishingly concludes (p. 130) that "the country and the Finnish political system were unusually healthy."

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GORDON A. CRAIG. *The Germans*. New York: G. P. Putnam's Sons. 1982. Pp. 350. \$15.95.

This book by Gordon A. Craig is both an informative introduction to the politics, society, and culture of West and East Germany today and a reliable, concise, and well-proportioned interpretation of major topics in German history reaching back to Tacitus and the Middle Ages but concentrating on the modern period. It consists of fourteen loosely connected chapters, from an outline of German history to reflections on "The Awful German Language." There are essays on religion, women, money, and soldiers in Germany's past and present. Craig deals with "Germans and Jews," "Professors and Students," "Democracy and Nationalism," and other topics. The book will appeal to readers outside the historical profession; it is written in a clear, even elegant prose, without footnotes and lengthy displays of scholarly erudition. But the specialist of German history as well will enjoy this combination of scholarly solidity and essayistic brevity, will discover things unknown to him before, and will find the volume useful for teaching purposes.

Craig is very familiar with his subject because of a lifetime of study and many personal encounters with German life; indeed, an account of his first encounter in 1935 introduces the book. But Craig also writes as an observer from the outside, and he makes good use of the relative distance and the comparative perspective implicit in this position. In spite of all his familiarity with German affairs, he maintains the ability to be a bit surprised about some of them, and this proves to be productive.

Craig presents a liberal interpretation of Germany's past and present. With usually implicit comparison with the West, he carefully stresses some German peculiarities: the destructive consequences of the Thirty Years' War and the resulting weakness of the middle class; the vitality of religion, partly in a very "rational" form; the limited impact of the Enlightenment and, correspondingly, the strength of philosophical and political romanticism, which later led to cultural pessimism; the belatedness of German nation building; the relatively late start and high speed of industrialization within an authoritarian political structure that resisted modernization; and the consequent deep social divisions and sharp ideological tensions. Craig stresses the continuity of certain traditions (for example, romanticism, antisemitism, the illiberalism of German universities, and militarism) from the nineteenth century to National Socialism. Although he perhaps underestimates the deep influence of World War I on German society, Craig convincingly argues that those long-term burdens of German history were decisively weakened or destroyed by the victory and the defeat of the Nazi dictatorship and by World

War II and its aftermath. He also notes the particular problems of the Federal Republic: problems of legitimacy and an insufficiently developed sense of proportion, both on the side of some protest movements in the "romantic" tradition and on the side of overreacting elites. (Surprisingly, Craig overreacts as well by equating the West German government's attempts to bar active communists from civil service positions in the 1970s with Metternich's suppression of oppositional intellectuals in the 1820s [pp. 188, 295].) But basically he emphasizes the achievements and stability of West German democracy. Bonn is not Weimar, and in most respects the Federal Republic has become a part of the West. In contrast, Craig stresses the authoritarian structure of the East German system. Perhaps he underestimates the long-term tensions implicit in the national question, that is, the existence of two German states in an age that does not seem to be moving beyond the principle of the nation-state. This may well be the beginning of a new German *Sonderweg* ("separate path").

On the whole, Craig's interpretation is convincing and forceful. It gives to National Socialism the weight it deserves in a synthesis of modern German history. Although it is quite clear that the German record would look different if compared with, let us say, Russia, it is legitimate (and, of course, natural for Craig) to choose Western countries as a basis for comparison. But I would like to make three qualifications. First, there are important elements in the German record that, in a comparative perspective, should be stressed more. The bureaucratic tradition is one such element. The strong role of the early established civil services deeply influenced many dimensions of German society, including the early development of the welfare state, quite in contrast to the Anglo-American world. Second, there are old and new foes of the liberal interpretation of German history (see, for example, Theodore S. Hamerow's review article in the *AHR*, 88 [1983]: 53–72). Craig's book will not convince them because—in spite of its comparative perspective—it stops short of explicit, systematic, international comparison. For example, Craig holds that discrimination against women in Germany was "more stubborn and protracted than in the advanced Western countries" (p. 147); only systematic comparison could prove the point. He writes about the extraordinarily high speed of German industrialization, but systematic comparison tends to raise doubts about this view (see, for instance, Hartmut Kaelble, "Der Mythos von der rapiden Industrialisierung in Deutschland," *Geschichte und Gesellschaft*, 9 [1983]: 106–18). Third, Craig is a master at using the stories of important persons and significant incidents to shed light on structures and processes. This makes for excellent reading. Figures are rare in his account. He is at his best when utilizing his profound

familiarity with German culture—music and architecture, and philosophy and literature from Gryphius to Grass—for interpreting German history. But the book is limited when it comes to the history of work and family structures and to the patterns of social inequality, mobility, and economic growth. There is nothing on demography, and the lower classes are conspicuously absent. Is this the price to be paid for the elegance of the narrative? It may well be that a comparative study of these important dimensions of historical reality will modify the present view of the German divergence from (and partial return to) the West, which is, after all, the central theme of Craig's impressive book.

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GARY D. STARK and BEDE KARL LACKNER, editors. *Essays on Culture and Society in Modern Germany*. Foreword by LEONARD KRIEGER. (Walter Prescott Webb Memorial Lectures, number 15.) College Station: Texas A&M University Press, for the University of Texas, Arlington. 1982. Pp. xi, 200. \$19.50.

This volume of essays, well edited by Gary D. Stark and Bede Karl Lackner, is a handsomely produced collection that will interest a broad audience because of the topics discussed and the methodologies employed. The essays all bear in some way on the relationship of culture to social class or social structure.

In "Culture and Society in Modern Germany" David B. King argues that Germany diverged culturally from Western Europe following the Thirty Years' War. After subsequently moving closer to Western development as a consequence of modernization during the nineteenth and early twentieth centuries, Germany diverged again under the Nazis, but resumed a convergent course in 1945. King remarks wistfully that this convergence has now gone so far that Germany no longer stimulates the West, as it once did.

I was struck by two comments that King makes *en passant*. Referring to contemporary Germany, he interjects that West Germany is "the only Germany, for better or worse, with a valid claim to the German past" (p. 41). This abrupt exclusion of the German Democratic Republic and perhaps much of the rest of Central Europe (such as German-speaking Switzerland, Austria, Bohemia, and the Baltic region) from the topic oversimplifies the issues that King raises about direction and continuity in German history. A second casual remark reflects a critical assumption about Germany's social development. Noting the Social Democratic party's support for the war effort in 1914, King writes: "The remarkable evolution of the Social Democrats . . .

from revolution to reform to war credits would seem to indicate that the underpinnings of the German social structure were reasonably sound" (p. 37). The Social Democrats' support of the war effort is a dubious standard by which to evaluate the health of the social order in Germany. I was so jarred by King's assertion that long after reading it I could still hear echoes of the late George Hallgarten lecturing fellow historians on the rottenness of the German bourgeoisie he was born into at the turn of the century.

Gordon Craig's essay on Theodore Fontane and Heinrich Mann presents another view of the German social order. Craig argues that both novelists were exceptional since Germany has had few "works of social realism or authors who combined high aesthetic standards with gifts of political and social analysis" (p. 98). He goes on to suggest that even Heinrich Mann's work was often deficient in aesthetic qualities. Craig does not develop his assertion that the social novel in Germany was weak because the country lacked an appropriate bourgeois audience for a German Dickens or a German Zola.

Using archival sources, Gary D. Stark in "Cinema, Society, and the State: Policing the Film Industry in Imperial Germany" demonstrates that Wilhelmine governmental authorities viewed the cinema with apprehension until its manipulative potential became apparent during World War I. The authorities long feared that the lower-class audience in the movie house would be incited to crime, violence, and subversion by what they saw on the screen. Stark's evidence is voluminous but limited almost exclusively to the opinions of state officials, some of them lower echelon. He does not explore the pre-war attitudes of other ruling-class circles toward film; evidence for these attitudes might, of course, be difficult to come by. Stark speculates that not until the Nazi regime was the manipulative potential of film that Ludendorff and others had perceived earlier actually exploited. One wonders whether there was not much more continuity in the socially integrative and manipulative uses of the German cinema than his speculation acknowledges.

David L. Gross's "*Kultur and Its Discontents: The Origins of a 'Critique of Everyday Life' in Germany, 1880-1925*" is a finely constructed attempt to explore the origins of the analysis of popular culture that culminated in the work of Georg Lukács, Walter Benjamin, Siegfried Kracauer, and, more generally, the Frankfurt School. Gross emphasizes the congruence in the nineteenth century of the suppositions of conservative or reactionary critics of contemporary culture like Paul de Lagarde and Julius Langbehn with the starting points of left-wing critiques of nineteenth-century popular culture. Gross depicts Georg Simmel as the seminal figure in the flowering of left-wing cultural criticism in the

Weimar Republic. Gross's thesis downplays the significance of Hegelianism and Marxism, but he leaves this controversial theme undeveloped.

The discussion of Nazi songs by Vernon L. Lidtke in "Songs and Nazis: Political Music and Social Change in Twentieth-Century Germany" should serve to stimulate dialogue and research on a neglected area. He finds that until 1933 the lyrics of Nazi songs expressed brutality, a readiness to struggle, and a hatred of opponents. Although differing from Socialist and Communist workers' songs, the Nazi songs were often derived from them. Melodies were taken over unaltered but lyrics were modified. Lidtke interprets this derivation as a consequence of Nazi endeavors to recruit workers. From 1933 until World War II the new Nazi songs became less pugnacious. Emphasizing national solidarity, they manipulated the populace for the imperialistic objectives of the regime.

Based largely on the memoirs of German scholars, Charles E. McClelland's "*The Wise Man's Burden: The Role of Academicians in Imperial German Culture*" presents a sensitive picture of the German professoriate during the Bismarckian and Wilhelmine eras. Although these memoirs constitute a rich source, McClelland's evidence for his claim that the professoriate felt a growing sense of isolation and dismay as their cultural standards were ignored or challenged by other bourgeois is drawn almost entirely from representatives of two disciplines, history and classics.

All of the work in this collection is fragmentary or incomplete. Although this statement is intended as a mild criticism of most of the essays and the many loose ends they leave us with, it is also complimentary. None of the contributors can pursue within the confines of a short essay all of the stimulating issues he raises. Some of the contributors tap hitherto neglected sources and open up new areas of research.

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HUGO KUNOFF. *The Foundations of the German Academic Library*. Chicago: American Library Association. 1982. Pp. xiii, 220. \$15.00.

The eighteenth century was not a golden age of the German university. It was, however, a prosperous age for the book. In the hundred years after 1700 book production increased fourfold, but the enrollment of students at universities substantially declined. Increasingly, it was being asked whether universities in their existing form fulfilled a worthwhile function. Should they not be transformed into training institutes, leaving serious research to academies of science? Two new foundations, however—

Halle (1694) and Göttingen (1737)—pointed the way to the future. Particularly in the case of Göttingen, the library, in a way that had not been the case before, was perceived to have a function central to the mission of the university.

To examine this development and to describe the methods devised by librarians to fulfill this mission is the task Hugo Kunoff has set for himself in this useful book. He has chosen to concentrate on the four Protestant universities that by common consent had a "clear superiority" (p. xii) in the eighteenth century—namely, Halle, Leipzig, Göttingen, and Jena. Their distinctive features are well described on the basis of conscientious research. Göttingen provides the centerpiece of the book, and Baron Gerlach Adolf von Münchhausen, the Hanoverian Geheimrat who presided over its founding and guided its destinies during its first thirty-five years, is the most imposing figure. Eager to see the "celebrity" of the new university quickly and firmly established so that it would lure students in substantial numbers, not least of all young noblemen and foreigners, Münchhausen relied heavily on the fame of productive scholars to accomplish this purpose; after a slow start a distinguished faculty was indeed assembled. A main function of the library, in his view, was to make Göttingen attractive by providing professors with the tools they required. A booklover himself, Münchhausen year after year found special funds for the library and busied himself with the details of its operations. The great German librarians of the eighteenth century were scholars of exceptional repute. This was conspicuously the case at Göttingen, where the renowned classical scholars, J. M. Gesner and C. G. Heyne, headed the library for seventy-three of its first seventy-five years. When, four years after Heyne's death, George Ticknor arrived at Göttingen in the first contingent of American students to enroll there, he was amazed at the richness of the library and the ease with which students could borrow books.

Kunoff is himself a librarian and his book is particularly valuable for the extensive attention it gives to the more humdrum aspects of the eighteenth-century library's operations—where it got its money, how collections were assembled, and how workable systems were devised for classifying material, cataloguing the books, serving the needs of readers, and so on. Less satisfactory are his introductory pages where sureness of touch in dealing with the broad intellectual and cultural tendencies of the times is lacking.

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HILDE HAIDER-PREGLER. *Des sittlichen Bürgers Abendschule: Bildungsanspruch und Bildungsauftrag des*

Berufstheaters im 18. Jahrhundert. Vienna: Jugend und Volk. 1980. Pp. 536.

The rise of the bourgeoisie is often measured in terms of its growing influence on the formation of both public opinion and public mores. The institutions involved in forming such attitudes include the church, schools, political parties, newspapers, literature, and, not least, the stage. Hilde Haider-Pregler has not focused her study of the eighteenth-century attitudes to German and Austrian theater on the political content of the dramas. Instead she has given us a brilliant analysis of the changing ideas of the role of the theater as a social and cultural institution. The influence of English and French Enlightenment writers is also sketched in. The main emphasis, however, is on German and Austrian thought of the period between 1730 and 1776. Included are such writers as Gottsched, Lessing, Justi, and von Sonnenfels. Omitted are Klopstock, Herder, and Goethe.

It was primarily the utilitarian ethic of German economic writers, cameralists like Seckendorff and Justi, that opened the Protestant mind to the usefulness of the theater in shaping public morals. As a *Sittenschule* it could be used to uplift the poorer classes, but only if censorship remained. Few argued for either freedom of expression or for a national theater before the 1750s, when van Swieten and Sonnenfels influenced the loosening of censorship in Vienna.

All eighteenth-century governments insisted on the practice of civic virtue (*Tugend*) and held the state responsible for maintaining it. Since moral leadership was still largely in the hands of the clergy (both Catholic and Protestant), criticism of the theater was aimed against "decadent" aristocratic tastes as well as against the lower-class humor (*Pöbelwitz*) of the market place. These were to be replaced with a dominance of bourgeois ideals. This tendency was dubbed "reform" theater, a movement that is traced to 1776, when Joseph II founded the *Burgtheater* as a national theater. Gottsched's influence in Viennese circles is traced through the letters, located by the author in the Leipzig University library, that he exchanged with students he placed there.

Chapter 2, "Church and Theater," is the most interesting for the social historian. The theater remained an object of attack for the Protestant clergy until well beyond 1700. By 1769 a Göttingen theologian was able to believe that a "purified" theater could be pleasing to God. Unless brought under censorship and clerical control, the professional stage was identified as a corrupt influence, promoting loose morals, adultery, and prostitution. In Dresden in 1724, Engelschall described the German-language theater as the "sinful humor of pagan spectacle" (p. 83). The Catholic clergy, especial-

ly the Jesuits and Benedictines who supervised the schools, were less rigid, even if many still cited the hostility of the Church Fathers to pagan theater. The Jesuits encouraged school dramas with moral content, performed, however, in Latin.

The reform theater demanded by the best authors of the day was intended to improve the spoken language. Coarse humor and illiterate, extemporaneous dialogues were condemned. A national stage was to create a national culture under upper-middle-class supervision. In 1730 Gottsched hoped that even the day laborer would be able to come to such a theater, already attended increasingly by the bourgeoisie. Under Francis I and in Theresian Vienna such ideals were realized with the emperor and the masses in attendance at the German comedy. All classes were to be led by reason to morality through the effect of live performances by morally educated actors. Prejudice, sex, and other forms of sensationalism were to be banned.

The author is able to conclude that the social status of the professional actor improved during this century not the least because absolute monarchy saw the importance of using the stage to provide an educational function as well as private entertainment. In the end, differences in dialect between northern and southern Germany remained. By implication one infers that this prevented the formation of a truly "national" stage under Austrian imperial leadership.

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GUSTAVO CORNI. *Stato assoluto e società agraria in Prussia nell'età di Federico II.* (Annali dell'Istituto Storico Italo-Germanico, number 4.) Bologna: Il Mulino, 1982. Pp. 479. L. 25,000.

Historians have long ceased to portray the eighteenth-century Prussian state as the dynamic, progressive institution once so beloved in Hohenzollern hagiography. The shift toward a more critical appraisal of the Hohenzollern monarchy was already visible in the twilight years of Wilhelmine Germany, when Otto Hintze and Johannes Ziekursch began focusing on the socioeconomic structures that informed and circumscribed monarchical institutions. Subsequent studies by Walter Dorn, Hans Rosenberg, and Hubert Johnson further dispelled the myth of the Prussian bureaucracy as a socially transcendent, efficacious instrument of the royal will.

Gustavo Corni's detailed examination of the relationship between the state and the countryside in eighteenth-century Prussia attempts to drive yet another nail into the coffin of the Hohenzollern legend. At the core of his analysis is the contention that the monarchy, far from being a dynamic agent

of economic transformation, positively inhibited Prussian agrarian development. Part I describes the bureaucratic obstacles and interference facing the progressive, economically vigorous class of domain leaseholders. According to Corni, the state's reluctance to extend long-term domain leases, as well as its clumsy, paternalistic efforts to protect the peasantry, only impeded the shift to wage labor and capitalist agriculture. The strait jacket of royal agrarian policy, Corni argues in part 2, similarly hampered the capitalist transformation of the noble estate. The monarchy's unwillingness or inability to extend credit to the peasantry neutralized the effectiveness of royal paternalism, while efforts to prevent the expropriation of peasant land (*Bauernlegen*) restricted the nobility's economic options. Attempts by Frederick II to prevent the bourgeois purchase of noble estates thwarted rural investment and drove penurious nobles even deeper into debt. All in all, Corni concludes, the primacy of the army and the fisc in royal agrarian policy produced a stifling obsession with social equilibrium. By the late eighteenth century, mounting noble indebtedness and rural proletarianization confirmed the bankruptcy of the monarchy's static social vision.

Although based entirely on published sources, this book is a valuable synthesis of recent scholarship in the field of Central European agrarian history. Recent East German and Polish interpretations are summarized with a particularly admirable clarity. Above all, Corni's work serves as a reminder of the yawning chasm separating royal decrees from their implementation. Doctrinaire and abstractly conceived policies invariably proved impractical or irrelevant when applied at the level of the village or estate.

The virtues of Corni's analysis are also its vices. He frequently hovers around historiographically controversial issues (the problem of the second serfdom, for example) without clearly stating his own position. Nor does his contention that the Prussian monarchy retarded the transformation of rural Prussia jibe with his view that Prussian agrarian policies were ineffectively implemented. If royal directives were diluted or even ignored in the countryside, then surely there were more important reasons for the belated development of Prussian agrarian capitalism. A host of local and regional studies will be necessary before those reasons are adequately understood.

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ROBERT EBEN SACKETT. *Popular Entertainment, Class, and Politics in Munich, 1900–1923.* Cambridge: Harvard University Press, 1982. Pp. viii, 194.

Robert Eben Sackett set out initially to study Munich folksingers, notably the popular Karl Valentin (Valentin Ludwig Fey) and Weiss Ferdl (Ferdinand Weisheitinger), in the late nineteenth and early twentieth centuries, but shifted his emphasis and now maintains that "the [book's] most important arguments involve their customers, Munich's tradesmen, shopkeepers, office employees, salespersons, lower-level officials, and their families" (p. vii). It is unfortunate that Sackett made this shift because the book's strength is in the treatment of the two entertainers themselves. Its serious flaw is in the author's sweeping generalizations about the audiences.

Following a brief introductory discussion of entertainment in Munich and of some methodological issues, Sackett examines a number of themes in the humorous presentations of Ferdl and Valentin. He then shows a relationship between those themes and the early lives of Valentin and Ferdl, stressing that for several years before the First World War "both fell from the traditional middle-class into the lowest ranks of the performing business" (p. 44). But differences between the two outnumbered their similarities, especially after 1914. While Valentin lacked sympathy for superpatriotism, expressed antimilitarist views, and, by the postwar era, "was a pacifist in an armed camp" (p. 133), Ferdl strongly endorsed militarism, antisemitism, conservative paramilitary organizations, and, finally, Adolf Hitler and National Socialism. Sackett's account of the two entertainers, their lives and their sentiments, is informative and contributes considerably to the history of popular culture in Munich.

The serious trouble lies with Sackett's contention that he has also made a substantial contribution to our understanding of Munich's middle class. Despite repeated admissions that he lacks documentation on the audiences, he stubbornly insists that Valentin and Ferdl expressed the sentiments of Munich's middle class because these two were the most popular among hundreds of entertainers. Deducing the opinions of audiences from what performers and speakers say is a dubious endeavor under any circumstances, but it is even more problematic when the performers are folksingers and humorists. It is plausible to assume some kind of correlation, but evidence, methodological rigor, and demonstrable differentiations are necessary if the attributions are to be persuasive. Sackett constantly imputes ideas and emotions to people he cannot identify and about whom he has little or no information. In view of the contrasting outlooks of Valentin and Ferdl, this approach leads to insoluble problems because there is no empirical data on which to differentiate clearly one audience from another. So he appeals to further speculations, for example, that some of Valentin's audiences probably did not un-

derstand that a particular presentation was antimilitarist and that another audience did (at the *Kammerspiel*) and was probably sympathetic because it was likely to have been upper class! Sackett uses the language of class so loosely and ascribes attitudes to unidentified people so casually that one can only recommend that readers take the social commentary in this book with an ample supply of skepticism. This is especially regrettable because it could have been a truly fine monograph on popular culture in Munich, if only the author had shunned the temptation to try to transform it into a study of social class and the rise of National Socialism.

VERNON LIDTKE

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KARL ROHE, editor. *Die Westmächte und das Dritte Reich, 1933–1939: Klassische Grossmachtrivalität oder Kampf zwischen Demokratie und Diktatur?* (Sammlung Schöningh zur Geschichte und Gegenwart.) Paderborn: Ferdinand Schöningh. 1982. Pp. 231. DM 32.

This volume unites an introduction by Gustav Schmidt and a conclusion by Karl Rohe with national surveys by Schmidt and others on the policies of Britain, the United States, and France toward Hitler and toward each other. With one exception, the essays stem from papers delivered at the Thirty-third Conference of German Historians in Würzburg in March 1980. Several of the contributors are political scientists, and the studies tend to emphasize the structural factors behind policies, such as industrial obsolescence, foreign trade concerns, class anxieties, party alignments, and the groups holding political and economic power. The writers stress the preoccupation of national leaders with the Great Depression, which could only tighten the interrelations among domestic, foreign, and defense policy. Many of the essays also comment incisively on the risks of drawing analogies between Western policies toward Hitler and policies toward the Soviet Union today.

Although the writers conferred before the conference, the articles show healthy differences in outlook and approach. Schmidt and Bernd-Jürgen Wendt both point out Britain's overextended security responsibilities and sensitivity to disruptions to foreign trade, but Schmidt attributes to British leaders an overriding concern with the survivability of the domestic socioeconomic system, while Wendt underlines London's mistaken hope that economic appeasement would strengthen the "moderates" in Berlin. Callum Alexander MacDonald, sympathetic to the ideas of William Appleman Williams, contends that the Open Door policy made the United

States, in the world economy, a "revisionist" opponent of British power; Klaus Schwabe specifically criticizes this view, holding that Franklin D. Roosevelt placed questions of security first. Gilbert Ziebura brings out the severe problems of the French economy, while Roland Hühne develops the theme of the disintegration of the social basis and political structure of the Third Republic. Unfortunately, some of the writers (particularly Wendt and MacDonald) tend to refer the reader to their own book-length studies for documentation. The stimulating collection is supplemented by a bibliographical essay.

Perhaps the most impressive contributions are Schwabe's balanced and convincing treatment of American policy, Ziebura's and Hühne's insightful discussions on France, and Rohe's outstanding concluding essay. Rohe has a gift for questioning assumptions and stereotypes. He argues, in particular, that neither classic great-power rivalry nor ideology can fully explain Western policy. For example, British leaders were deaf to Hitler's bids for an alliance, bids well worth considering from the viewpoint of pure *Realpolitik*, but they also were most reluctant to think in terms of democracy versus dictatorship. They and the other national leaders were guided by complex traditions and unspoken assumptions; by different attitudes toward war, territorial rule, and the state; and by different ways of life. Indeed, the alienation of Germany from the West had long been developing. In short, Rohe argues for the continuity and conditioning power of national cultures and goals. As one who has tried to call attention to such influences on both the financial and the security policies of the powers, this reviewer readily agrees with Rohe's views.

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BERND WEGNER. *Hitlers Politische Soldaten: Die Waffen-SS, 1933–1945; Studien zu Leitbild, Struktur und Funktion einer nationalsozialistischen Elite*. (Sammlung Schöningh zur Geschichte und Gegenwart.) Paderborn: Ferdinand Schöningh. 1982. Pp. 363. DM 48.

The observation that books dealing with some facet of the Third Reich are among our craft's more common artifacts has become a truism. Bernd Wegner has written such a book, but it is of uncommon quality. This work is not an addition to the often tendentious bibliography of Waffen SS battlefield exploits and excesses. The reader will search in vain for a discussion of the SS Panzer Corps's tactical role in the recapture of Kharkov or a re-creation of the horror of Oradour-sur-Glane, nor will she or he find photographs of Tiger tanks rumbling through the Ardennes. In fact, the combat history of the

Waffen SS has only incidental relevance to what is, primarily, an ideological, structural, and social analysis of that organization.

Wegner's book comprises five major components, each a tightly woven analysis of a fundamental aspect of the growth and development of the Waffen SS. Parts 1 and 2, focusing on ideology and organization, offer relatively little that is new. Part 3, "Instruction and Education," is more rewarding. Here, at last, is an adequate treatment of the SS-*Junkerschulen* (officer training academies) and their curricula, vital for an understanding of the attitudinal qualities and technical preparation of the lower and middle segments of the Waffen SS officer corps. The large numbers of officers trained by the prewar *Junkerschulen*, many more than were required by the modest scale of the peacetime armed SS establishment, suggest to Wegner that Himmler was actively planning a mammoth expansion of the militarized SS long before the outbreak of war. The wartime growth of the Waffen SS, therefore, was not simply an opportunistic response to the demands of an increasingly desperate conflict.

The social composition of the Waffen SS officer corps is examined in part 4. This analysis, based largely on a judicious sampling of dossiers in the Berlin Document Center, reveals the existence of a basic division or, perhaps more appropriately, a generation gap between those older, relatively senior officers, who had joined the SS following careers in the pre-1933 army or police, and younger, more politicized officers of lower but steadily advancing rank, whose careers were rooted in the SS. Nor was the bifurcation limited to age and experience. With some notable exceptions, senior officers possessed antecedents in the upper middle class, which were similar to those of much of the army's officer corps but which were in contrast to the predominantly lower-middle-class origins of junior ranks. The officer corps of the wartime Waffen SS, then, was clearly in transition from unavoidable compromise with an older order to realization of a new, politically committed nobility of the sword. At the same time, ironically, the enormous proliferation of Waffen SS divisions during the war rapidly diluted the ideologically and technically elite character of enlisted ranks, a point effectively developed by Wegner in the book's concluding section.

This is an intelligently conceived, superbly researched book. It is indispensable reading for all serious students of the "SS-State."

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MARTIN VAN CREVELD. *Fighting Power: German and U.S. Army Performance, 1939–1945*. (Contributions

in *Military History*, number 32.) Westport, Conn.: Greenwood Press. 1982. Pp. xi, 198. \$27.50.

Martin van Creveld has produced yet another provocative book that like his previous one on logistics from Wallenstein to Patton (*Supplying War* [1977]), is bound to stimulate discussion. Whereas logistics may be seen, on the whole, as a life-saving preoccupation, this time the young military historian from the Hebrew University of Jerusalem has decided to deal with fighting power, which he defines "as the sum total of mental qualities that makes armies fight" (p. 3). Predictably, the book's motto is Napoleon's often-quoted dictum that "in war, the moral is to the physical as three to one." Fighting, of course, means killing, and van Creveld's book, in essence, is about how the Nazi German armed forces, the Wehrmacht, earned its epithet of excellence in this unsavory social activity.

With the aid of almost sixty tables and figures van Creveld conducts a sophisticated analysis of measurements and calculations, juxtaposing the Wehrmacht to the U.S. Army in order to establish where the secret of the former's superior efficiency lay in scoring more kills than the enemy. Indeed, on a man-for-man basis the Wehrmacht on average inflicted casualties at a 50 percent higher rate than the opposing Anglo-Americans, regardless of whether on the attack or the defense and whether numerically superior or, as was more often the case, heavily outnumbered. After indulging perhaps in too many superlatives concerning the excellence of the Wehrmacht in his introductory chapter, van Creveld proceeds in a more sober and systematic way to look into a wide range of categories: social status, structure and mobility, army organization and administration, rewards and punishments, and the role of noncommissioned officers and of the officer corps. He scornfully rejects the assumption that the so-called German national character was more warlike than others because of its perhaps excessive preoccupation with orderliness (van Creveld's quip on page 13: "the result of toilet-training at too early an age"), nor does he regard indoctrination in itself as the main backbone of an efficient fighting force. Consequently, van Creveld is reluctant to consider the Nazi web in which the Wehrmacht found itself competing with the vastly expanding Waffen SS, whose fighting power was even more superior. (Here I must express my astonishment in finding only one unimportant passing reference to the SS in the whole book, as if there were no relationship between the two. How dangerously misleading to the specialized Western military historians of the 1980s who might gradually get accustomed to both the positive assessment of the Wehrmacht's fighting power and the convenient obliteration of the SS presence!)

And yet, van Creveld's own tables on the death sentences and executions for all offenses, including desertion, cast a gloomy light on the price paid for the "greatest possible effectiveness in combat": while between 1914 and 1918 a total of 102 death sentences had been carried out by the German army, for 1939–45 the figure comes close to 12,000 (excluding the Waffen SS, I presume). Although the relative desertion rates for the U.S. army in Europe, which van Creveld uses for 1944 and 1945, exceeded the German figures several times over, only one death sentence on a U.S. soldier (Private Eddie Slovik) was ever carried out. This, van Creveld admits, "brings out some very deep differences in their respective philosophies" (pp. 113–17).

Thus, in concluding, while one would like to accept van Creveld's assertion that fighting power has retained its decisive significance even in today's world of technological progress, one should also readily accept his warning that the extremely high fighting power of the German army had been extracted at the cost of producing troops who could with almost equal ease not only wage a world war but "commit any kind of atrocity as well" (p. 173).

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MICHAEL WORTMANN. *Baldur von Schirach: Hitlers Jugendführer*. Cologne: Böhlau. 1982. Pp. 270. DM 38.

Michael Wortmann's study makes clear, as no more self-conscious study could, how National Socialism could be totalitarian and yet in no way monolithic. Baldur von Schirach learned early on that Hitler would accept facts accomplis and used that knowledge to expand his power. He continually pressed the borders of his Hitler Youth organization up against those of other agencies and institutions. The *NS-Frauenschaft* wished to establish a feeder organization among girls; Schirach headed them off by extending membership in the *Bund deutscher Mädel* to age twenty-one. He long resisted the introduction of preparatory military training into the Hitler Youth (this with Hitler's approval) in order to ward off any influence of the Wehrmacht on youth. When Hitler in January 1939 suddenly gave the SA a role in premilitary training, Schirach concluded a detailed agreement with the army. He was capable of all sorts of intrigue. When war resulted from Hitler's attack on Poland, Schirach presented himself to Hermann Göring, then to Josef Goebbels, and proposed that he replace Bernhard Rust as minister of education, thus uniting all aspects of education for the war effort. The quick German victory over Poland derailed that attempt. Schirach's

scramble for power became frantic as Germany mobilized. Other Nazi agencies might increase in importance with the war, but the experienced leaders of the Hitler Youth, always in short supply because of the ceaseless expansion of the organization, were now siphoned off for service in the Wehrmacht. Schirach himself joined the army and went to the western front, but was later recalled by Hitler and sent to Vienna as *Reichsstatthalter*, and *Gauleiter*, while still retaining some claim to the Hitler Youth.

In Vienna, Schirach became a patron of the arts. He also presided over the deportation of sixty thousand Jews. Despite Schirach's later disclaimers, Wortmann presents evidence that six months after the transfers had begun Schirach had been informed of just what deportation meant. Late in the war, after the deportations, Schirach appeared before a youth congress he had organized and made a violently antisemitic speech calling the deportations a "contribution to European culture" (p. 212). If we are to believe one account, he admitted the next day that the scandalous and brutal speech was made simply for political effect (perhaps merely to draw attention to the congress and further his ambitions). Wortmann regards that act of dangerous cynicism as completing Schirach's moral and political corruption.

Whether because of his closer contact with reality or his greater distance from Hitler's court, Schirach foresaw the impending disaster, and in 1944 he and his wife went to plead with Hitler for a less ruthless occupation policy in the east and for negotiations with the Allies. Hitler would have none of it and interrupted Henrietta Schirach when she mentioned their distress over the transportation of Jews from Amsterdam. The author wonders what took the Schirachs so long to notice the plight of the Jews, as the deportation of Jews from Vienna seems not to have aroused their concern. His wife suggested Schirach be replaced, but Hitler refused and sent him back to Vienna to preside as *Gauleiter* over its fall.

Schirach was sentenced to twenty years of imprisonment by the International Military Tribunal at Nuremberg, although the defense pleaded that there was no blood on the hands of the defendant. Schirach does stand out from the others around Hitler for his disgust with *Kristallnacht* and for his last-minute stirrings of conscience, faint though those may have been. But as long as things were moving forward Schirach was eager to take advantage of events. Though pessimistic about the war, Schirach became enthusiastic about the possibilities of looting Russia at one point, as vistas of power momentarily unfolded before his eyes.

This is not a case, if there were such, of a conscience inverted and set to evil ends, but of a

conscience deadened and for years enveloped in opportunism. The passage of time has not helped much with our understanding with the "Why?" of Nazism. Perhaps studies such as this one, which answer the "How?" constitute all the lesson we are ever going to derive from the events.

LAWRENCE D. WALKER
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GERHARD BENECKE. *Maximilian I (1459–1519): An Analytical Biography*. Boston: Routledge and Kegan Paul, 1982. Pp. xiii, 205. \$25.00.

Where modern biographers of Maximilian I are concerned, there appears to be no middle ground. In Austria, Hermann Wiesflecker has recently finished a study of the Renaissance emperor in five gargantuan volumes. Now Gerhard Benecke from England brings us a work so idiosyncratically selective that it hardly seems in touch with the complexities of a human life at all. Each author in his own way has decided that the Habsburg cannot hold his own as a subject but is to be understood only through the re-creation of the circumstances in which he lived. It is an altogether curious approach to a man whose gaudy personality and bent for psychological self-exposure would seem to compel the historian to focus on him rather than on his environment.

Benecke calls his work an "analytical biography," by which he means a number of things. Chiefly, however, these are an analysis of Maximilian's "style of government" and using Maximilian to illuminate the larger social, political, economic, and intellectual configurations that shaped both his age and him. Benecke has been laudably enterprising in exploring the administrative records of Maximilian's reign housed in the archives of Vienna and Innsbruck. His observation that there is much to be culled from these materials is certainly true; what he himself has done with them, however, is not very imaginative. He frequently uses long excerpts of documents or citations of them *in toto* as a substitute for thorough analysis of the problems and questions raised in them. So often does he employ this stratagem that one begins to think that one is reading an anthology rather than a unitary text.

The book is divided into fourteen relatively short chapters, most of which purport to deal with the impact Maximilian had on those under his control. These range from peasants and miners to his court entourage. The approach is not an unpromising one, but its execution leaves much to be desired. Treatment of the court, for example, is largely confined to the economic circumstances in which that institution functioned. Its cultural and educational roles do not appear to interest Benecke

greatly. The penultimate chapter on Maximilian's mental frame of reference has a lot to say about astrology and precious little about Christianity, an equally crucial part of the emperor's intellectual make-up. Benecke wishes to prove, among other things, that Maximilian was not as haphazard a financial administrator as conventional wisdom among historians would have him to have been. He never carries through with this, however, except to show that the Habsburg could make advantageous deals and insist his prerogatives, with force, if necessary. Rather, the picture one carries away from the book as a whole is that of a ruler who could never match appetite, in the extended sense of that term, with income. In other words, Benecke has succeeded only in resketching the traditional view of Maximilian, which he sought to correct.

Benecke is thoroughly at home in the social and economic history of Renaissance Germany. His choice of illustrations for the book, some of which, such as the portrait of Maximilian's sister, Kunigunde, are rarely reproduced, shows that he is knowledgeable in cultural matters as well. He has it in him to do a far better book than this. Perhaps he should jettison the biographical genre altogether and simply write about life and labor in Renaissance Austria. This reviewer, among many, would welcome it.

PAULA SUTTER FICHTNER
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RICHARD SCHOBBER. *Die Tiroler Frage auf der Friedenskonferenz von Saint Germain*. (Schlern-Schriften, number 270.) Innsbruck: Universitätsverlag Wagner. 1982. Pp. 606. Cloth \$7 50, paper \$6 80.

Although faced with many bitter disputes, the Paris Peace Conference wrestled with no territorial issue as complicated as the question of South Tirol. The principle of self-determination should have guaranteed a plebiscite, but Tirol's future was soon intertwined with numerous unrelated problems—Italy's imperialism in the Adriatic, its desire for "strategic and natural frontiers," the Austro-German *Anschluss* question, a possible Danubian confederation, the Anglo-American need to placate France and Italy, and the very continuity of the conference. Austria's confusion concerning its future orientation toward Germany, Italy, or the Entente complicated matters. In all this, the rights of the German and Ladin South Tirolese were quickly forgotten. Maybe the Tirolese were unrealistic in believing that a war as bitter as this one could end with the noble concept of self-determination. It was easier for the defeated to profess unqualified acceptance of the ideal than

for the victors, who had the difficult task of dividing the spoils.

Richard Schober has discussed the question of Tirol at the peace conference in this comprehensive and exhaustive framework, thus going far beyond the narrow confines indicated by the book's title. Even though he says that "the problem of South Tirol at the peace conference was caught between Wilson's principle and the Treaty of London" (p. 44), he makes it clear that much more was involved. Although the Tirolese people and their leaders seem to have hoped until late 1919 that the unity of Tirol could be preserved, it is clear on the basis of the author's exhaustive archival research that the decision to grant Italy the Brenner Pass was reached early in 1919 and that no possibility existed for a revision in favor of demands based on the area's German history and ethnic composition. Italy's demand, based on dubious arguments of strategic needs, was readily accepted by Wilson and the Allies, possibly because many even more controversial questions awaited solution. Although the author could have made these points within a very few pages, he examines all alternatives considered by the Tirolese, and thus produces an excellent documentary analysis, drawing on all pertinent archival sources. This in itself is worth the reader's attention. The author also adds an appendix of about one hundred fifty pages of significant documents.

Schober is a bit harsh in his treatment of the Austrian Social Democrats, their pro-*Anschluss* position, and in particular of Foreign Minister Bauer, but this reviewer cannot quarrel with the overall conclusions. Schober emphasizes that "all forces involved, the political leaders of North and South Tirol, but also the national government, had made an effort, though at times with inadequate means, to save South Tirol" (p. 442), and he credits all Austrians involved with sincere motives. Some emotional involvement by the author in a question that is not yet fully settled is clearly visible and admitted.

This reviewer is not aware of a comparable work on this topic, although some aspects, such as the Treaty of St. Germain and the *Anschluss* question, are treated in greater detail elsewhere.

FREDERICK DUMIN
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MELANIE A. SULLY. *Continuity and Change in Austrian Socialism: The Eternal Quest for the Third Way*. (East European Monographs, number 114.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1982. Pp. xiv, 288. \$25.00.

An updated history of Austrian socialism in English was hoped for by many a student both of socialism

and of recent Austria. To a large extent Melanie A. Sully's work fulfills this hope. It follows the career of Austrian socialism from before World War I through the two republics into the early 1980s. Throughout the book the emphasis is on internal continuity and change rather than on the ties of the party with other parties, with foreign countries, or with nonsocialist groups, episodes, and phenomena. This contrasts with many other publications, for example, recent Austrian books on Bruno Kreisky, in which associations between the socialists and the general history of the country are at a premium.

Sully devotes two chapters to the party prior to 1918, two chapters to the first republic (one of which is on the Revolutionary Socialists after 1934), and four chapters to the second republic, a somewhat unbalanced scheme of things at the expense of the interwar years, despite the longer time span of the second republic; for to show the great differences in party philosophy between the first and second republics, she should have given more space to the two decades before World War II. On the positive side, however, she must be given credit for a painstaking treatment of the dozen years of sole executive rule by the socialists (1970–82), especially for her exploration of the tug-of-war between those who wanted a restructuring of society, like Josef Hindels and the left wing, and those who wanted more attention paid to immediate practical aims and a "mass party," with Kreisky himself somewhere in the middle but tilting toward the pragmatists. Sully is also to be applauded for the extremely helpful graphs and maps (why does she call them illustrations, though?), the very useful appendixes, and a full-bodied bibliography.

A few shortcomings cannot be overlooked, minor though each of them may be. The subtitle, *The Eternal (!) Quest for the Third Way*, does not seem to be overly felicitous. The title of the last chapter, "The SPOe in Government," covering the years 1970–82, appears strange in view of the inclusion of the Austrian Socialist party in coalition governments before 1970.

More importantly, no matter how professionally valuable a book may be, if it has a certain amount of typographical and grammatical errors and reveals inadequate proofreading, the discriminating reader is made to feel slightly uneasy.

A final objection: on page 212, one reads that the SPOe "shamelessly and effectively competed with the people's party for the moderate centre and 'liberal' voters." Why shamelessly? Should that word not, perhaps, be reserved for the socialist effort to court former Nazi votes soon after 1947 rather than for the innocuous effort to court the moderates?

Despite these "strictures" the student of Austrian socialism must respect this important contribution to the historical literature of postimperial Austria.

The chapters on party history since 1945 must be particularly esteemed for their tightly packed information. The years between 1945 and 1970, which saw the growth of the party from "loyal opposition" to exclusive carrier of executive responsibility, are difficult to describe. Sully has not only done it well but has also mastered the challenge of showing the delicate balance between continuity, tradition, socialist idealism, and radicalism on the one hand, and utilitarian flexibility on the other hand.

Scholars interested in the continuity versus change theme in Austrian socialism should also note that the historical congress of April 1982 in St. Poelten was devoted to this issue and that minutes of that congress are printed in *Oesterreich in Geschichte und Literatur*, 1982, number 4.

ROBERT SCHWARZ

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CARLO M. CIPOLLA. *The Monetary Policy of Fourteenth-Century Florence*. (Publications of the UCLA Center for Medieval and Renaissance Studies, number 17.) Berkeley and Los Angeles: University of California Press, 1982. Pp. xiv, 112. \$14.95.

Carlo M. Cipolla is the master of the short historical monograph. Navigation, clocks, literacy, coinage, epidemics—all difficult subjects—have been exposed to the master's pithy and often penetrating analysis. To these topics, now he adds a ninety-two-page discussion of Florentine monetary policy in the fourteenth century. Students of Florentine history will find here a lucid summary of work already published by others. For his materials on economic developments, particularly for an outline of the mid-fourteenth-century banking crisis, Cipolla draws on Saporì's unsurpassed studies; fiscal developments are documented by reference to Barbadoro's and Barducci's works; information on wages and prices comes primarily from de la Roncière's vast and detailed investigations; the data on which monetary policy itself is analyzed is found in Bernocchi's four-volume compilation and in his own analysis of Florentine monetary history; finally, the framework of political history is borrowed largely from Brucker's important books. In short, in vain will one look for new information in this book. The facts on which Cipolla's analysis is based have been well known to scholars.

The originality of this little book, quite beyond the author's *bravura* in synthesizing work available to him, is the result of Cipolla's largely successful effort to interpret monetary policy not only as a consequence of political decisions but also as a result of economic forces evident in the marketplace. The monetary policy of Florence has often been seen by historians of that city in a very narrow, specifically

Florentine context. Fascinated by the plethora of documents regarding the city's history found in the *Archivio di Stato*, historians have tended to view the periodic devaluations of silver currency in the fourteenth century as a consequence largely of political decisions by groups that at one time or another controlled their city's politics. Cipolla very convincingly enlarges the context within which these devaluations should be interpreted. Thus the devaluations of the mid-1340s, in the wake of the banking crisis, are juxtaposed to the concurrent and unrelated loss of value of gold, while the devaluation of 1371 is quite illuminatingly interpreted in light of the massive influx of cheap Pisan coins into the Florentine market. The discussion of the events of 1371 very neatly illustrates the operations of Gresham's law. On the other hand, the monetary stability of the 1350s and 1360s—a period when, following the demographic catastrophe of mid-century, wages improved—seems itself to have been the consequence not only of complex economic forces but also of the increased power that new social groups had obtained in Florentine society.

Written in his customarily lucid and elegant prose, Carlo Cipolla's new book represents a useful contribution to the study of Florentine history. If, as is true, the author is successful in widening the background against which monetary policy should be interpreted, it is equally true that, perhaps unwittingly, he has provided some support for those scholars who, more recently, have tended to emphasize the vastly unequal distribution of political and economic resources in late medieval Florence. For a reading of this book will convince one that, regardless of the causes of the periodic devaluations, normally it was the entrepreneurial elites in the city who were most able to take advantage of the changing monetary policies of their government, thereby augmenting their power.

ANTHONY MOLHO
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JUDITH C. BROWN. *In the Shadow of Florence: Provincial Society in Renaissance Pescia*. New York: Oxford University Press. 1982. Pp. xxv, 244. \$29.50.

Few scholars of the Italian Renaissance have ever visited the Valinievole, a small valley tucked into the foothills of the Apennines, forty miles northwest of Florence. But in the last five years two Valdinievole communities have been the subjects of a pair of American dissertations and books, Frank McArdle's study of Altopascio, the rural estates of the Medici grand dukes, and, more recently, Judith C. Brown's study of Pescia, the bustling market town that dominated the valley. That two such detailed and thorough investigations of provincial Tuscan econo-

my and society are even possible is a tribute to the Florentines who kept such meticulous tax and estate records for lands under their dominion. It is also an indication of a shifting focus among present-day Florentinists away from the dominant city toward local studies in the provinces.

Brown's study of Pescia from the fourteenth to the seventeenth centuries seeks to consider the extent to which the town's economy was affected by the loss of independence and absorption into the Florentine state. After a difficult period in the late fourteenth and early fifteenth centuries, when Florence was frequently at war and fiscal demands on citizens and subjects alike were particularly high, Pescia began to enjoy an economic revival culminating during the period of the Medici grand dukes in the late sixteenth century and lasting until the 1630s. Brown attributes Pescia's growth to the town's ability to adapt to the demands of a new interdependent regional economy by specializing its agricultural production in silk, wine, and oil rather than cereals. Economic initiative and its profits belonged to a small number of patrician entrepreneurs who were able to buy out small farms to consolidate their land holdings, introduce the specialized crops, and bring in paper and silk industries. Pescia's economic good fortune in the late sixteenth century has led Brown to conclude that subject communities in the Florentine state fared much better under the rule of the grand dukes, who made lighter fiscal demands and developed a Tuscan rather than just Florentine economic policy, than they had under the republic.

The book is less explicit about the broader social implications of these economic changes outside the patriciate. Since the study ends just when Pescia's economy had peaked, we can only speculate about its downward spiral in the seventeenth century. Certainly McArdle's study of nearby Altopascio presents a rather dismal picture of the long-term effects of the Florentine dukes' policy on their Valdinievole estates. Like Pescia, Altopascio had enjoyed a brief period of relative prosperity and demographic growth at the end of the sixteenth century, when Ferdinando I encouraged peasants to immigrate and clear new land for cultivation, some of it probably in mulberries to supply Pescia's silk industry. But the long-term effects of ducal policy were to cause the proletarianization of agricultural labor and its steady impoverishment. Pescia was probably spared some of the bleakest consequences of Medicean policy because a much larger portion of Pesciatine lands and wealth stayed in private hands and because the town remained a market center. The comparison of Altopascio and Pescia leaves us to ponder the interesting question of whether in fact Pescia prospered by virtue of being in the shadow of Florence or despite it. A definitive

answer will have to await additional local studies of other communities within the Florentine dominion. One can only hope they will be as carefully conceived and crafted as Brown's study of Pescia.

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CESARE MOZZARELLI. *Sovrano, società e amministrazione locale nella Lombardia teresiana, 1749–1758*. (Saggi, number 220; Storia e Amministrazione, number 1.) Bologna: Il Mulino, 1982. Pp. 235. L. 15,000.

In his *Settecento riformatore* (1969, 1976), Franco Venturi identified reform as the single major trend in eighteenth-century Italy. From Piedmont to Sicily rulers set about demolishing the legal and administrative encrustations of centuries to start the process of governmental modernization. The degree to which reforms were enacted varied from state to state, depending on the commitment of the ruler and his ability to enforce changes that undermined tradition and privilege. Such efforts were most successful in Tuscany and Lombardy, both swept by the reforming spirit of the Habsburgs.

Lombardy had barely passed from Spanish to Austrian rule after the War of the Spanish Succession when Charles VI ordered a census of property and inhabitants in his new province, but fundamental reforms had to wait for the reorganization of government under Maria Theresa and Joseph. For Maria Theresa the mainspring of these reforms was the crown's financial crisis after the disastrous warfare that had inaugurated her reign. For Joseph, a doctrinaire faith in reform itself provided the impetus. Mother and son concurred in wanting to create an efficient administration in which the state's authority would be centralized in, and emanating from, Vienna. In Lombardy, Habsburg reforms were ably carried out against bitter opposition from vested local interests by three Italians: Gianluca Pallavicini, Pompeo Neri, and Beltrame Cristiani, all highly competent professional administrators, chosen by Maria Theresa for their talents rather than for their pedigree.

This study by Cesare Mozzarelli focuses on the crucial decade following the end of the War of the Austrian Succession, during which Neri, on orders from Vienna, initiated the complete reorganization of the Lombard administration, introducing order and uniformity where there had been chaos and diversity. As preparation for the fiscal reforms that would ensure a stable and secure source of income to Vienna, Neri was ordered to update the earlier census. It soon became apparent that no fiscal reform could establish an equitable and enforceable

tax system throughout the crazy quilt of provincial and communal governmental structures without introducing sweeping administrative reforms. Accordingly, in 1755 legislation provided for reorganization of the almost fifteen hundred Lombard communes listed in the census. Between 1756 and 1758 there followed a series of ordinances reordering the administrative structure of the region's provinces and major cities. Although these reforms aimed at uniformity and centralization, governance was left in the hands of local citizens, charged with implementing Vienna's directives. Despite the promise of limited self-government, the reforms met with obstinate resistance from tax-farmers, the clergy, and most of the patriciate. Mozzarelli traces these conflicts and the changes eventually imposed on the communal administrations of Cremona, Pavia, Casalmaggiore, Como, Lodi, and finally Milan itself. By the end of the 1750s, the main reforms had been put into place. Rid of its cumbersome, multiple administrative bodies and inefficient tax structure, Lombardy acquired the foundation from which a modern society would evolve.

Based on exhaustive archival research and wide-ranging familiarity with German and Italian secondary literature on the issues, this work illustrates an important aspect of the "reforming century." Specialists in the field will find it interesting and useful.

EMILIANA P. NOETHER
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GIANNI TONIOLO, editor. *L'economia italiana, 1861–1940*. Foreword by ALBERTO CARACCIOLLO. (Libri del Tempo Laterza, number 168.) Bari: Laterza e Figli, 1978. Pp. xv, 381. L. 5,500.

The second edition of the volume *Lo sviluppo economico italiano, 1861–1940* (1973) differs so substantially from the original that in some ways it is a new book. Certainly, much more than the title has been changed. The changes were prompted by contributors who reconsidered their findings and conclusions in light of subsequent research and publication, as well as the altered perspective of the editor, Gianni Toniolo. Since the debate on the "new economic history" has subsided, Toniolo discarded the section on methodology found in the first edition, further transforming the structure of the volume. Thus a number of articles in the present work are new, others have been substantially revised, while a number present in the first edition have been discarded.

After the short introduction to the new edition by Alberto Caracciolo and Toniolo, there follows Caracciolo's preface to the 1973 edition, which remains largely unchanged. This introductory sec-

tion of the volume is completed by two essays: the first by Toniolo, who teaches economic history at the Ca' Foscari in Venice, and the second by Giorgio Fuà, who teaches political economy at the Ancona branch of the University of Urbino. Together they provide a narrative and statistical survey, of Italian economic development for the period under consideration, placing what follows in perspective. Fuà's compendium of statistics, drawn from his *Formazione, distribuzione e impiego del reddito dal 1861: sintesi statistica* (1972), is useful, while Toniolo's assessment of Italian economic developments from unification to 1940 remains one of the best compact treatments of the subject. Toniolo, combining facts and analysis, transcends the factual approach of Luigi de Rosa's survey, "L'Italia economica," found in Corrado Barbagallo's *Cento anni di vita italiana* (1948).

The text is divided into two parts, with five articles in the section on the formation of economic Italy and three in that on the myths and reality of the Fascist economy. Part 1 includes two articles by Stefano Fenoaltea, who teaches economics at Duke University. Published originally in English, they concentrate on public policy and the stages of economic growth in Italy from the *Risorgimento* to the Great War and on the role of railroads in Italy's economic development during the same period. They are more thematic and theoretical than the work of Toniolo, which precedes them, and different in focus from the essay on Italian monetary policy by Pierluigi Ciocca and that on population by Ercole Sori, which follow; but there is considerable overlap. Indeed, many of the issues covered in these pages have already been examined in collections such as Caracciolo's *La formazione dell'Italia industriale* (1963) and Giorgio Mori's *Il capitalismo industriale in Italia* (1977). The interrelationship between industrialization and education, considered by Vera Zamagni of the University of Trieste, has received far less attention; and Zamagni's piece therefore makes an important contribution.

The lead essay of part 2 is contributed by G. M. Rey of the Ancona branch of the University of Urbino. Rey's work provides a synthesis and overview of the Italian economy during the Fascist decades and is followed by the more sharply focused articles of Jon S. Cohen of the University of Toronto and Giuseppe Tattara of Ca' Foscari, which concentrate respectively on the revaluation of the lira in 1927 and the "battle for wheat" launched by the Fascist regime in 1925. Both authors stress the political motivation underlying Fascist economic decisions and cite the need to examine Fascist policies within the broader political and international framework. In this section methodological problems and ideological issues combine to render the conclusions concerning Italian economic develop-

ment during the Fascist years far more tentative than those made for the Liberal age.

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MECHTHILD GOLCZEWSKI. *Der Balkan in deutschen und österreichischen Reise- und Erlebnisberichten, 1912–1918*. (Quellen und Studien zur Geschichte des Östlichen Europa, number 16.) Wiesbaden: Franz Steiner. 1981. Pp. vi, 288. DM 52.

This monograph by Mechthild Golczewski is an analysis of published diaries, memoirs, and travel impressions of German and Austrian observers and participants in the Balkan Wars of 1912–13 and the First World War in its Balkan theater. The primary object is to discover to what degree cultural bias and nationalist prejudice were reflected in the portrayal of various peoples with whom the foreigners came into contact. Not too surprisingly, they were found to be influential indeed.

Such mental stereotypes as "the Prussians of the Balkans" (the allied Bulgars), "tattered sheep-rustlers" (the hostile Montenegrins), and "fatalistic Muslims" (the allied but irritating Turks) abound in the sources employed by Golczewski, some one hundred fifty otherwise forgotten individuals. The large majority of them were military men, total neophytes to the Balkans and its history, languages, and culture. In view of this and the reasons and conditions of their presence in southeastern Europe, any other attitude toward the locals would have been astounding.

This dissertation at the University of Cologne is an eminent example of that academic species, the book that should not have been written. It is difficult to conceive of any useful purpose it may serve either in the scholarly community or for the general public. It adds nothing of interest to the understanding of events or human nature. The duly footnoted pages conclude with such profundities as "the general characterization [of the Balkan peoples] is particularly dependent on whether they are friends or foes" (p. 269)! The quality of this insight will be first fully appreciated when it is recalled that its sources are the public utterances of war journalists and militarymen, during or just after the decisive conflict of their lifetime.

With all due cognizance of the *Publikationszwang* imposed on German PhDs, still a point can be reached—is reached here—when the conventional wisdom (and the dictates of common sense) are best accepted without further validation by laborious rehashing of the previously known.

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LAJOS LUKÁCS. *The Vatican and Hungary, 1846–1878: Reports and Correspondence on Hungary of the Apostolic Nuncios in Vienna*. Translated by ZSÓFIA KORMOS. Budapest: Akadémiai Kiadó. 1981. Pp. 795. \$48.00.

Lajos Lukács's *The Vatican and Hungary, 1846–1878* is a highly specialized collection of over three hundred letters sent to the Vatican by the apostolic nuncios in Vienna during the pontificate of Pío Nono. The letters are not translated from the original Italian, but they are preceded by a two hundred-page, English-language text in which Lukács discusses them in their historical setting.

Taking advantage of the opening in 1966 by the Vatican of its archives for the period of the reign of Pius IX, Lukács has published a series of letters that, he writes, "makes it possible to survey Hungary's historical development from a special angle . . . [that] of the basic interests of the Roman Curia" (p. 17). A second reason for the book, according to Lukács, "is to disclose the main links behind this special point of view, [and] analyse the socio-political ideological factors which led to such conclusions and concepts" (p. 17).

At the outset, Lukács states that his book is more a political history of Europe in general and Hungary in particular than a church history. Neither he nor the papal nuncios tell us anything strikingly new about social history or the evolution of nationality conflicts in the turbulent years between 1846 and 1878. Some of the intricacies of Vatican politics, however, are elucidated by Lukács's book. For example, he seeks a more balanced view of the 1855 Concordat, which has been seen by some liberal historians as a major victory for an obscurantist papacy against the enlightened forces of modernization.

Lukács uses the nuncios' letters to show that signing a concordat is not the same as implementing it. The magnitude of the papal victory of 1855 and the subsequent setback for Rome in its nullification after 1867 have both been exaggerated, Lukács claims. In its campaign against the legacies of Vienna's Josephinism, Rome fought on many fronts with mixed results in the years of Pius IX (p. 103).

The strength of the book is the insight it provides into the reporting of the nuncios back to Rome. Lukács selects those communiqués that dealt in some way with Hungary, where the nuncios were meticulous observers of the passing scene but, with the limitations of their own seminary educations, hardly profound in their assessments of political and social change.

Antoniacci Mariano Falcinelli, named nuncio in Vienna in 1863, worried about the Protestant Count Friedrich Ferdinand von Beust entering the government in 1866, then expressed his reassurance by Beust's pledge to respect the rights of the church in

the monarchy. Falcinelli, nonetheless, went along with Rome's inflexible stand precluding compromises with the liberals in Vienna, who eventually nullified the Concordat of 1855 (p. 163 and document number 243, p. 602). In his condolences to Franz Joseph on the death of his brother Maximilian in Mexico shortly thereafter, Falcinelli reminded the Austrian emperor that the church was the strongest support against revolutionaries and free-masons (document number 247, p. 610).

Rome faced a problem in dealing with the Hungarians that it was unable to resolve. The nuncios were aware of the need for popular support among the Catholics of Hungary for the church. This meant, however, concessions to a growing Magyar nationalism that was weakening the Habsburg monarchy, itself a trusted and needed ally for a papacy trying to contain Italian nationalism and preserve its secular domain. The progressive weakening of the monarchy during the thirty-two year reign of Pius IX was evident to the nuncios who had no successful prescriptions to offer. The limited vision of the bureaucrats of Pius IX in Vienna is made absolutely clear by Lukács's collection.

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GYULA TOKODY. *Deutschland und die ungarische Räterepublik*. Translated from Hungarian by HANNELORE KRÜGER. (Studia Historica Academiae Scientiarum Hungaricae, number 183.) Budapest: Akadémiai Kiadó. 1982. Pp. 129. \$8.00.

This modest volume argues that opportunism rather than ideological considerations underlay German-Hungarian relations of the time of the Hungarian Soviet Republic in 1919. The Germans feared Bolshevism, but they wished to safeguard their troops and investments in Hungary, whereas Hungary needed German coal shipments and moral support at the peace conference. Gyula Tokody surveys German press, political, and diplomatic opinion about Hungary to show the vast range of positive and negative reactions to Hungary's communist regime.

Generally, Tokody finds that most Germans preferred a weak Hungary unable to block German eastward penetration or complicate German-Czechoslovak relations and dependent on German support. The Germans feared that if Communist Hungary cooperated with Soviet Russia, Romania and Bulgaria would soon turn Bolshevik and desert the German economic orbit. Furthermore, the communist regime was ideologically and politically hostile, mistreated Hungary's German minority, and might corrupt Germany's working classes. With the exception of the Communist party, nearly all

German political factions rejected Hungarian communism and believed that Germany should avoid Bolshevism as a means of forcing the Entente into granting concessions.

At the same time, however, Tokody discovers that most Germans considered Hungarian Bolshevism a blessing in disguise. The right-wing German press, for example, thought of it as concealed nationalism. Fear of this communism would scare the victors into building a German anti-Bolshevik bulwark. Count Franz Fürstenberg-Stammheim, Germany's consul general in Budapest, urged his government to maintain good relations with Hungary, because, whether large or small, it could serve as Germany's transit route to the Near East, and Hungary's German minority could influence the Magyars to adopt a pro-German orientation. The Germans were particularly cheered by Hungary's continuing dependence on German coal and by the Entente's hostility; these would drive Hungary into Germany's arms and encourage communist moderates in its government.

Tokody maintains that Hungary benefited only indirectly from German actions. German intransigence on the eve of Versailles may have prevented direct military intervention in Hungary by the great powers, but the Romanians were not deterred from launching their successful April attack. Germany's hostile neutrality strengthened the Entente's anti-Hungarian hand in Central and Southeastern Europe, whereas a pro-Hungarian German course, in his view, would have facilitated a Russo-German rapprochement and materially improved Germany's international position. Tokody doubts whether any German behavior could have rescued Hungary's communist regime from collapse.

Tokody seldom strays from the path of scholarly detachment, and he avoids Marxist jargon. The book has no table of contents, chapters, bibliography, or index; but Marxist and non-Marxist secondary sources adequately document this microcosm of post-World War I Central European historiography.

THOMAS SPIRA

University of Prince Edward Island

EILA HASSENPFUG-ELZHOLZ. *Böhmen und die böhmischen Stände in der Zeit des beginnenden Zentralismus: Eine Strukturanalyse der böhmischen Adelsnation um die Mitte des 18. Jahrhunderts.* (Veröffentlichungen des Collegium Carolinum, number 30.) Munich: R. Oldenbourg, for the Collegium Carolinum, Munich. 1982. Pp. 464. DM 58.

Conventional wisdom has it that the victory of the Catholic League at the battle of the White Mountain in November 1620 destroyed the liberty of the

Bohemian estates, and from then until the Napoleonic wars a period of darkness descended on the lands of the crown of St. Wenceslas. This concept of nineteenth-century Czech historiography is gradually being dismantled. The work under review, a slightly revised dissertation under the guidance of Karl Bosl, marks a giant step forward in this process.

Eila Hassenpflug-Elzholtz really attempted two things, which she then skillfully wove together to answer a question that she had set herself, namely, why during the War of the Austrian Succession so many Bohemian nobles gave their allegiance in 1741 to the Bavarian Elector Charles Albert and deserted the Habsburg claimant to the Bohemian throne, Maria Theresa. The first part of this exhaustive work is an account of the survival and evolution of the corporative organs of the Bohemian estates since the battle of the White Mountain; the second is a structural analysis of the Bohemian estates, particularly of the two noble groups, the lords and knights, in 1741–42. She chose the accession of Maria Theresa as the most suitable point in time, for once again the estates were put in the difficult position of having to make a choice of allegiance. The needed documentation came from the archives of the Bohemian court chancellery created by Charles Albert, now in Munich, and the private archives of the Kinsky family in Vienna.

What the author found was that the corporative principle and the institutions of the estates had been so deeply entrenched in Bohemia that the Emperor Ferdinand II failed to build up those strong centralized organs that elsewhere in Europe had become the most effective means for establishing absolutism. Hence the estates continued to exert much more influence than surmised. The ill-famed *Verneuerte Landesordnung* of 1624 had limited the legislative competence of the estates, but they still retained the right to approve taxes. Beyond this, the noble estates virtually monopolized the staffing of the central administrative organs and had control over the local and district governments. In fact, the creation of a permanent executive organ, the *Landesausschuss*, in 1714 had strengthened the position of the estates. The attempt to integrate the Bohemian nobility into the Habsburg monarchy as a whole by settling foreign noble families in the kingdom after White Mountain miscarried over the long run; for the newer families tended to identify themselves with the interests of their new lands. Nevertheless Emperor Leopold I opened up to the nobles opportunities for entering Habsburg service, of which 26.5 percent of them had availed themselves by 1741. This led to a division in the ranks of the nobility, at times crossing family ties. While a powerful block within the nobility, who still nurtured the hope of consolidating the power of the estates after the death of Emperor Charles VI, saw

their opportunity at the coming of Charles Albert, those in Habsburg service generally remained loyal to Maria Theresa.

Hassenpflug-Elzholz has worked out her thesis in great detail with a considerable amount of analytical study. More than two hundred pages are devoted to listing all the individual members of the Bohemian estates, giving data about their landholdings, tax liabilities, service and officeholding, and their attitude toward Charles Albert. Although the book is not easy to read, it will undoubtedly become basic to any future study of Bohemia, in the seventeenth and early eighteenth centuries the key province of the Habsburg dominions.

HANNS GROSS

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SARAH MEIKLEJOHN TERRY. *Poland's Place in Europe: General Sikorski and the Origin of the Oder-Neisse Line, 1939-1943*. Princeton: Princeton University Press. 1983. Pp. xvi, 394. Cloth \$40.00, paper \$15.00.

In this study Sarah Meiklejohn Terry undertakes the tangled history of the London Polish emigré government during World War II and its efforts to secure a future boundary on the Oder and Neisse rivers. The author points out that General Władysław Sikorski, a De Gaulle-like leader, but in some ways unlike the French general, became aware very early in the war that Poland would not return to its pre-1939 status. Terry also analyzes Sikorski's policy toward the Allies; she rightly points out that his strong faith in the Allies, which included Soviet Russia, contributed to the dissension and disunity among the London Poles (pp. 54-55).

In spite of considerable research, *Poland's Place in Europe: General Sikorski and the Origin of the Oder-Neisse Line, 1939-1943* has major limitations. Over 80 percent of Terry's material comes from emigré sources, yet even this documentation is incomplete. She portrays the archival situation in 1976 Poland in very grey political terms. This reviewer's experience in the same time frame was the exact opposite. I found the Polish archival staffs to be friendly and helpful. Materials to September 1939 were generally all open. If the author had problems with the *Archiwum Ministerstwa Spraw Zagranicznych* (Archives of the Ministry of Foreign Affairs), she could have turned to the other Polish archives. The archives and the library of the *Instytut Zachodni* (Western Institute) are probably the best source on the Oder-Neisse line. The research work of the *Instytut Zachodni* goes back long before World War II. During the interwar period extensive studies were made there at the behest of the Polish government covering the Silesian, East Prussian, and Oder river regions, thus, detailed maps with Polish historical

place names were already published in the mid-1930s. The Piłsudski Institute of New York City has some of these maps. The archives of this important emigré center were not consulted by Terry.

In her bibliographical research Terry often includes books that are not scholarly works. Among her sources are some semifictional historical accounts such as *A Man Called Intrepid* by William Stevenson (1976) and *Native Realm: A Search for Self-Definition* by Czesław Miłosz (1978). In addition, she does not consider certain standard historical books on the subject. Missing from her bibliography is *Polska Granica Zachodnia* (The Polish Western Border) by Gerald Labuda (1971), which contains over seventy pages of bibliography.

The following studies are also not included: *Germany and Poland* by W. W. Kulski (1976), *Pogranicze polsko-niemieckie jako pogranicze etnograficzne* (The Polish-German Borderland as an Ethnographic Borderland) by Zofia Staszczak (1978), *German and Pole* by Kenneth Rosenthal (1976), *Polski Ruch Narodowy w Niemczech* (The Polish National Movement in Germany) by Wojciech Wrzesiński (1970) and *Germans, Poles, and Jews* by William W. Hagen (1980). Among more outstanding works on the subject, the author bypasses the studies on the Oder-Neisse and the Baltic borders by Casimir Smogorzewski and by Alfons Klafkowski and Bolesław Wiewióra.

This type of bibliographical research causes some deficiencies in Terry's presentation of the historical figures and interpretation of concurrent events. For example, Terry never fully explains the origins of Sikorski's policy on the Oder-Neisse border. To state that the followers of Piłsudski were dominated by the chauvinistic nationalism of Roman Dmowski (p. 24) is simply not true; it is from this milieu that a follower of Piłsudski, Sikorski, emerged. Papers concerning the future movement of Poland's boundaries westward were presented to General Sikorski by Jan Kaczmarek, Stefan Murek, and Arka Bożek, members of the Polish *Rodło* organization made up of Poles living in Germany, in Paris in 1939-40. In addition, after the fall of France, Tadeusz Sulimirski, a major instigator of the Oder-Neisse line idea, organized a West Slav study group in Crawford, Scotland, in the early part of 1940. This group had close contacts with Sikorski. Sulimirski is only mentioned twice in the footnotes by Terry, while his two co-organizers, Zygmunt Sławiński and Stanisław Połujan, are not cited at all. A memoir of this important West Slav study group and its early contacts with General Sikorski are given in great detail by Jerzy "Jur" Lerski, a member of the group, in "Szkocka kontrofensywa Zachodniej Słowiańszczyzny" (Scottish Counteroffensive of Western Slavs) *Symposiones* (1978).

In summary, *Poland's Place in Europe* does not tell the whole story. Although it is an important first

step and is extremely ambitious, the complicated origins of Sikorski's policy and the origins of the Oder-Neisse line have not been recounted in proper historical detail.

RICHARD WOYTAK
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CHARLES A. RUUD. *Fighting Words: Imperial Censorship and the Russian Press, 1804–1906*. Toronto: University of Toronto Press. 1982. Pp. viii, 327. \$30.00.

Charles A. Ruud's volume integrates a survey history of publishing in Russia with a study of the policies and instruments of state censorship during the century before the revolution of 1905. Based on a wide range of unpublished materials in Soviet and American archives, Ruud's account further examines the rapid growth of Russia's periodical press during the second half of the nineteenth century despite the efforts of recalcitrant imperial censors to slow its progress.

Historians sometimes overemphasize the place of radical writers in Russian journalism and letters; Ruud's work reminds us that such men were but a handful of those writing during the last century of the tsarist regime. Perhaps more important, his findings show that the periodical press and book publishing flourished to a remarkable degree during the half-century before the revolution of 1905 and that, as the twentieth century opened, these had grown too powerful for the autocracy to control through traditional instruments of censorship. No longer could the emperor and his agents hope to suppress well-founded criticisms of Russian life and politics. By 1905 even Russia's radical press had come under the rule of law, administered apart from the autocrat's personal preference and whim. The result, Ruud concludes, was that "Nicholas II had as his only weapons against the radical press the force of law wielded by the judicial system" (p. 207).

But the history of book publishing and journalism during the last century of imperial Russia's history is only part of Ruud's tale. Throughout the nineteenth century Russia's emperors and statesmen tried to insulate readers from works they thought morally or politically dangerous and to control the development of public opinion within the empire. Therefore the goals of Russian censorship, and the instruments with which its defenders tried to realize them, form the second large dimension of Ruud's study, although this is less satisfactorily dealt with than the first. Ruud pays little attention to the development of the institutions of censorship within the broader context of Russian bureaucratic politics, especially during the years of the great reform era, and his discussion of Russia's political processes during those years takes too little account of the new

interpretations offered by Terence Emmons, Daniel Field, Richard Wortman, and Daniel Orlovsky, just to name a few of the scholars whose writings have reshaped our views about the great reform era during the past fifteen years. Further, Ruud passes over the debates against *glasnost* (publicity) that seized the attention of bureaucratic reformers at that time, although these were important in shaping their attitudes toward censorship.

Yet the shortcomings of this volume ought not to rest on the author's shoulders alone, and one should register at least a token protest against publishers who give their authors less than the best. Certainly the University of Toronto Press has done Ruud no great service in this book's physical appearance or in the quality of its editing. It is a minor irritation to find some first names used in their Russian forms while others are Anglicized, and even minimal editorial effort could have eliminated a number of awkward turns of phrase that mar the book's text. Nor is it too much to ask that a bibliography of secondary works be included in a work of this length and scope. Nonetheless, this is still a helpful book, especially for students of nineteenth-century Russian journalism and bookpublishing, and it can also serve as a useful guide to the richness of Soviet archival materials on those subjects for others interested in further research.

W. BRUCE LINCOLN
Northern Illinois University

W. BRUCE LINCOLN. *In the Vanguard of Reform: Russia's Enlightened Bureaucrats, 1825–1861*. DeKalb: Northern Illinois University Press. 1982. Pp. xix, 297. \$23.00.

One of the most familiar questions in nineteenth-century Russian history has always been, how was it possible for a conservative autocratic regime to formulate and to carry out, not only the emancipation of the serfs, but a whole series of other far-reaching measures that have come to be known collectively as the "great reforms"? All of the reforms, but above all the emancipation of over twenty million privately held serfs from what was in every respect true slavery, required much more than a stroke of the pen. Realistic knowledge of the problems to be confronted, as well as legal and legislative skill and experience, were essential. In a country where public discussion of social and political issues was forbidden almost to the eve of the actual reforms, how was it possible for anyone, even officials, to gain the requisite knowledge, experience, and, above all, the will to act?

Everyone concerned with imperial Russian bureaucracy already knows that W. Bruce Lincoln has been developing a comprehensive answer to many

of these questions in a series of some twenty articles and books that have appeared in the past fifteen years. We now have the full story in one volume, and it must be considered one of the most important works of modern Russian social history yet to appear. It is a study of the "enlightened bureaucrats," a small portion of the top-level mid-nineteenth-century officials who for no more than twenty years, probably less, played a crucial role and then faded quickly from the scene after the assassination of Alexander II in 1881.

The book begins with what is certainly the best general description of the nature of the bureaucratic system under Nicholas I that I have seen. There, and throughout, the author has used a wide range of published and archival sources and he has been particularly successful in finding and using manuscript memoirs of former officials. Amid the rigid, dreary, bureaucratic world of Nicholas I there were a few energetic and far-sighted men (Kiselev, Perovskii, and Panin) who played a crucial role in encouraging the development of a small cadre of well-trained and devoted young officials. Most notable were Andrei Zablotskii-Desiatovskii, Nikolai Miliutin, and Sergei Zarudnyi, who were given the opportunity to do worthwhile and interesting work in areas such as gathering statistics and drafting municipal government legislation. In turn, during the last decades of Nicholas's reign, these men developed in their departments a larger "second generation" of enlightened officials who were not far from their mentors in age and general worldview.

One of the most interesting and original chapters demonstrates the extensive informal contacts that these young officials had both with each other and with other shades of opinion in Petersburg. Like the more well-known radicals, they had their "circles," although their caution and reluctance to commit anything even slightly unorthodox to paper has made Lincoln's task in reconstructing them most formidable. For despite their recognition of Russia's social and economic problems, the enlightened bureaucrats were devoted, loyal supporters of the autocracy, though they were viewed as radicals, even called "reds," by traditionalists. They believed firmly that reforms must be made from the top down, according to the will of the autocrat, designed and implemented by men like themselves. They successfully opposed the efforts of gentry liberals who aimed at playing an active part in working out the terms of the complex emancipation settlement, and opposed the idea of a representative body of nobles, even one with the most limited powers.

The enlightened bureaucrats were important officials but they were not, in the main, part of the immediate circle of senior advisors to Alexander II. They were not part of the traditional court elite.

They did not make the decision to emancipate the serfs and embark on the other great reforms (although they helped create the atmosphere that made the decision possible)—the emperor made it. Once the decision was made their role was important, but even then they did not hold many of the very top positions. Indeed, Nikolai Miliutin was ousted from his position as deputy minister of the interior in 1861, for reasons that are really not discussed fully by Lincoln.

The only significant weakness in the book is in the last two chapters, which deal with the great reforms, and it reflects the ambiguity of the enlightened bureaucrat's role. It is impossible to focus both on the reforms and on the enlightened bureaucrats, because their role is only part of the story, making the discussion seem incomplete. Given the aim of the book as a whole this is probably inevitable, but, particularly for a nonspecialist reader, it leaves significant questions unanswered.

In a work of such a high level of scholarship, drawing on rich and varied sources, it is most regrettable that the footnotes, which add so much to the reader's understanding of the basis of the author's argument, have been relegated to the back of the book.

WALTER M. PINTNER
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A. A. LEVANDOVSKII. *Iz istorii krizisa russkoi burzhuanno-liberal'noi istoriografii: A. A. Kornilov* [A History of the Crisis of Russian Bourgeois-Liberal Historiography: A. A. Kornilov]. Moscow: Moscow University Press. 1982. Pp. 178. 1 r. 50k.

Aleksandr Aleksandrovich Kornilov (1862–1925) is best known to North American students of Russian history for his work *Russian History in the Nineteenth Century*, which was based on lectures delivered at the St. Petersburg Polytechnic Institute early in this century. Serious scholars of Russian history also are aware of his important and more specialized work on the Russian peasantry during the famine of 1892–93, social movements under Alexander II, Russian politics in Poland, the Bakunin family, and peasant reforms and social movements in the last century generally.

This little book by A. A. Levandovskii analyzes the three main themes of Kornilov's historical and political career by allocating a chapter each to his writing on reforms in Russia during the 1860s, especially serfdom; Kornilov's conception of Russian historical growth as it is depicted in his course of lectures; and his studies of the Bakunins. The book concludes with fairly extensive documentation (with regular, but often undated, references to archival materials), a chronological outline of Kornilov's life and work,

and a list of his publications. The book does not include an index of names or subjects.

Besides archival sources and Kornilov's main body of writing, the author has used several articles written by Kornilov between 1904 and 1917, his book reviews (especially of V. I. Semevskii), and some political pamphlets, the significance of which Levandovskii overstates somewhat. Kornilov's correspondence with friends from his student days, many of whom became noted scholars, and with well-known historians whom he met later (for example, P. B. Struve, P. N. Miliukov, and A. N. Pypin) was also used. Although there is a ritualistic nod to Lenin as the methodological inspiration for this book, the fixation with rigid interpretive categories that so often has undermined the credibility of Soviet historians who write about their predecessors is not a prominent feature of this study.

Levandovskii chides Kornilov for failing to praise the Decembrists and takes care to note his constant support for the Kadets. But such "failings" are described as normal consequences of Kornilov's own political views. Only when he outlines A. A. Kizevetter's criticisms of Kornilov's historical scheme does the author go beyond a subdued party-line interpretation of Kornilov to show that he himself disagrees with a Kornilov position. Levandovskii places Kornilov firmly within the nineteenth-century state school historical tradition, but says that he touched on a few fields enough so that later historians were able to create a new, more progressive historiography. Kornilov is "a clear illustration" of a general "crisis" in Russian bourgeois historiography at the turn of the twentieth century; that is, he tried but was unable to break completely with the school of thought with which he was most familiar.

Levandovskii does not convince me that there was, in fact, a "crisis" in "bourgeois historiography" in Russia at the turn of this century; he ignores completely certain characteristic themes of Kornilov's historical overview (for example, his clearly stated aversion to antisemitism and official Russification policies). All in all, however, this book is a useful addition to a growing list of Soviet studies on individual Russian historians.

J. L. BLACK
Carleton University

G. P. KUROPATNIK. *Rossia i SShA: Ekonomicheskie, kul'turnye i diplomaticheskie svyazi, 1867–1881* [Russia and the USA: Economic, Cultural, and Dipomatic Ties, 1867–81]. Moscow: Nauka. 1981. Pp. 372. 2 r. 40 k.

Western historians of Russian-American relations have long known that the Soviet Union encourages

its historians to explore topics that shed light on periods of rapprochement between the United States and Russia. This Soviet commitment has been rewarded by the development of thoroughly knowledgeable historical specialists, including the world's leading historian of early Russian-American relations, Nikolai N. Bolkhovitinov. With the publication of the work under consideration here, the Soviets appear to have launched another world-class historian in the person of G. P. Kuropiatnik.

Kuropiatnik, the author of several previous works on U.S. history and Russian-American relations, here deploys his historical arguments in seven artfully written chapters. His initial review of Russian-American relations in the 1860s notes the heightened friendship fostered by the Russian fleet visits of 1863, but says surprisingly little about the sale of Alaska. Russian-American trade rates a short chapter in view of the relatively low value of such commerce during the period in question. Chapters on social and cultural contacts and scientific and technical cooperation illuminate developments that, while interesting in themselves, lose significance when seen in a broader context. An exception is the Russian career of Eugene Schuyler, who translated Turgenyev and Tolstoi for an American audience while serving as U.S. consul at Moscow from 1867 to 1875. The strongest chapters in the book deal with diplomatic relations, particularly during the eastern crisis and the Russo-Turkish War (1875–78). Kuropiatnik's account of Russia's attempts to employ U.S. ports as safe havens for elements of its navy in times of crisis makes particularly interesting reading.

In terms of basic research, Kuropiatnik has visited the Diplomatic Branch of the National Archives as well as the Manuscript Division of the Library of Congress, the New York Public Library, the Kansas Historical Society, and the Bancroft Library at Berkeley. Soviet archives employed include AVPR and TsGAOR in Moscow and TsGAVMF and TsGIA in Leningrad. Kuropiatnik's bibliography is substantial and demonstrates familiarity with the major English-language sources. The book is furnished with subject-geographical and name indexes, always a pleasant discovery in a Soviet publication. All told, this is an important work that breaks new ground in a largely unexplored period in Russian-American relations.

Although the Soviet commitment to the study of Russian-American relations is commendable, it should not be forgotten that Soviet research efforts tend to concentrate on the "positive" aspects of bilateral relations. As a result, Soviet historical accounts lack balance, particularly regarding topics that lend themselves to alternative interpretations. We in the West should produce our own Bolkhovitinovs and Kuropiatniks, for only a thor-

ough, balanced knowledge of history will enable us to view U.S. relations with Russia, past and present, in correct perspective.

J. DANE HARTGROVE
National Archives

IU. B. SOLOV'EV. *Samoderzhavie i dvorianstvo v 1902–1907 gg.* [Autocracy and the Nobility in 1902–07]. Leningrad: Nauka. 1981. Pp. 255. 2 r. 10 k.

This specialized monograph reflects some of the least useful characteristics of contemporary Soviet historiography. It lacks a theme or even a point of view (other than a general Marxist-Leninist one); it fails to analyze the key issues of its topic; it presents pages of undigested excerpts from archival material; and it is written in a tedious, obscure, and dense style. Its purpose is unclear and, except for archival references, it can be of little help to either scholars or general readers.

Like Iu. B. Solov'ev's earlier study, *The Autocracy and the Gentry at the End of the Nineteenth Century* (1973), this book examines the important subject of relations between the tsarist government and its principal supporter, the nobility, in the autocracy's last years. Yet Solov'ev maintains a narrow, descriptive focus. He accepts uncritically the general Leninist proposition that the basic interests of the autocracy and the gentry coincided and that when the nobility, whether conservative or liberal, was at odds with the government, it was only to urge "window-dressing" reforms or other measures designed to suppress the revolutionary movement. Solov'ev makes no attempt to analyze political divisions or social differentiation among the gentry and he largely ignores the provincial nobility. For example, we learn nothing about the motives and objectives of the liberal opposition among the gentry before the revolution of 1905, a period covered in a long first chapter that makes up half the book.

Later, the gentry is treated as if it spoke with one voice, when in fact there were major disagreements and splits. Thus, no light is shed on the complicated and significant relations among Stolypin, the leaders of the United Nobility, and other elements in the years 1906 and 1907, a period treated in the third and last chapter of the book. As a recent study by Neil B. Weissman has shown, the political issues and relationships at this time were complex, and the gentry played a major role in blocking reform of local government.

The archival material used for the most part merely supports what is already known. For example, the author presents interesting detail concerning the political attacks against Witte by court circles and conservative nobles, but the role of the far right in undermining Witte's position is hardly news.

Since other recent Soviet works show that even in present circumstances historians in the USSR can offer challenging theses and original perspectives, it is particularly disappointing that in this case the efforts of a competent historian using a range of archival material and treating a major topic should result in so little of significance.

JOHN M. THOMPSON
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NINA TUMARKIN. *Lenin Lives! The Lenin Cult in Soviet Russia*. Cambridge: Harvard University Press. 1983. Pp. xiii, 315. \$20.00.

Secular cults devoted to heroic leaders have been one of the most obvious features of Communism. Not only have the two great powers venerated their own leaders, but the images of Lenin, Stalin, and Mao have been revered by foreign movements, in and out of power. In addition, there have been small-power claimants to cult status and the half-hearted glorification of Khrushchev and Brezhnev. In the background loom the larger-than-life images of Marx and Engels. Despite the well-established outlines of this phenomenon, apparently one of the defining features of Communism (for many years; we may have entered a postheroic era in this political culture), there is scarcely any systematic research on the matter.

Against this background Nina Tumarkin's study of the Lenin cult in Russia is an important pioneering work, the precursor, one hopes, of other studies of the phenomenon in its full scope. Because it is the first work of its kind, it is understandable that Tumarkin has delimited her research. She deals only with the Soviet Union, bypassing the complex question of the reception of the Lenin cult among Communists in other countries. This limitation raises an interesting question about the theme of Tumarkin's first chapter, a thoughtful discourse on the roots of the Lenin cult in Russia. It may well persuade that Russia was a particularly nurturing environment for leader cults, but there remains the question of why the cult was accepted so readily by Communists in very different national settings.

Another limitation is chronological. Tumarkin provides only a brief epilogue on the course of the Lenin cult after the late 1920s, when Lenin's image became entwined with Stalin's. It is understandable on practical grounds that she decided not to undertake a study of the Stalin cult, but I am not wholly satisfied with the argument that Tumarkin advances in the epilogue to justify the decision. On the one hand, she is certainly right in arguing that much of the Lenin cult became welded to the newer Stalin cult and subordinated to it. On the other hand, the

mausoleum and many established Lenin rituals remained in place, and a new edition (the fourth) of Lenin's works was initiated in the postwar Stalin period. And as she notes, but very briefly, the anti-Stalin campaign corresponded to a vast revival of the Lenin cult, outlasting its main reviver, Khrushchev.

Leaving aside these problems of definition, Tumarkin has illuminated admirably the handling of Lenin's heroic image in his active lifetime, during his final illness, at the time of his death, and in the few years following. This is the most meticulous and thoughtful treatment of the steps that led to the preservation of Lenin's remains and the construction of the building that houses them. The discussion of the process by which the vast, spontaneous outpouring of feeling at the time of Lenin's death was routinized and exploited politically also deserves high praise, as does this difficult and essential undertaking as a whole.

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NEAR EAST

MEHDI KEYVANI. *Artisans and Guild Life in the Later Safavid Period: Contributions to the Social-Economic History of Persia*. (Islamkundliche Untersuchungen, number 65.) Berlin: Klaus Schwarz. 1982. Pp. xii, 339. DM 64.

This book, as stated on page v, represents a PhD thesis submitted to the University of Durham in 1980. In the introduction, Mehdi Keyvani describes the difficulties of collecting economic data on the Safavid period, difficulties that arise in part from the destruction of Safavid records and documents after the fall of that dynasty and in part from the paucity of this type of material in the contemporary historical chronicles. Nevertheless, as Keyvani points out, much progress has been made since the publication in 1943 of Vladimir Minorsky's pioneering work on the *Tadhkirat al-Mulūk*. This progress has been achieved by the sort of painstaking combing of Persian chronicles that Keyvani has undertaken and by collating material thus obtained with that contained in the valuable accounts of contemporary European travelers in Iran. Keyvani's book is a welcome addition to our knowledge of the trade and craft guilds in seventeenth- and eighteenth-century Iran. It is extremely useful to have gathered together in one place information on the types of artisans employed by these guilds, on the internal organization of the guilds, and on the fiscal regulations that governed their operation.

There are, however, a number of points of interpretation of the data on which I would take issue with Keyvani. Space prevents me from mentioning more than a few of these.

Keyvani writes: "it is probably true to say that the popular notion of the Safavid period, particularly 'Abbās I's reign, being a period of economic and commercial prosperity is incorrect or exaggerated, and that the volume of the external and internal trade of Iran was less than that of many other countries at the time (e.g. India or Ottoman Turkey)" (p. 215). The comparison made in the second part of this sentence is pointless; in view of the size and population of the Ottoman and Mughal empires relative to the Safavid state, it would be strange if this were not the case, and the point is not in dispute. A more constructive (and instructive) comparison would be with the degree of economic prosperity in Iran both prior to and after the reign of Shah 'Abbās I. If this comparison is made, it is not just a "popular notion" but a well-attested fact that 'Abbās I increased the prosperity of the Safavid state to a level far exceeding that which existed at his accession. At that time (1588), Ottoman invasions of northwestern Iran had seriously impaired the Persian silk trade, and the Portuguese still exercised a stranglehold over the transit trade in the Persian Gulf, Arabian Sea, and Indian Ocean, and through Iran to Mediterranean ports. 'Abbās I's policies were directed essentially to increasing Iran's exports and its revenue from the transit trade. He greatly increased the volume of Iran's most lucrative export, silk, by making the silk trade a state monopoly. Keyvani is extremely critical of this and other interventions by the state in the economy and sees the monopolistic control of the economy by the state as being responsible for the failure of Iranian merchants "to evolve into an enterprising and influential capitalist class or bourgeoisie" (p. 216). This criticism, first made by Amin Banani, has some justification, but the fact is that *without* state support and organization, Iranian merchants would never have been able to meet foreign challenges in the seventeenth century. Keyvani himself admits that "it does not necessarily follow that such a class would have arisen in Iran if the Safavid state had pursued different policies" (p. 242). Keyvani disapproves, too, of 'Abbās I's encouragement of Armenians, which led to the latter's assuming a dominant role in Iran's international trade. But Persian merchants had proved themselves to be less adaptable to the needs of international trade, and they lacked the banking and credit facilities that the Armenians possessed.

On the one hand, Keyvani blames the "exploitative economic policy which the régime had long pursued" for inflation and a general deterioration of the economy (p. 121), yet on the preceding page

he states that, until the very end of the Safavid period, the rise in prices was "gradual," and he supports the latter statement with a comparative table of prices of essential commodities between 1581 and 1722. According to this, a hen in the time of 'Abbās I (1588–1629) cost 5 pence, and in the time of Sulaymān (1666–94), 6 pence. Hardly inflationary!

Throughout the book, there is an undercurrent of hostility toward the Safavid regime that is not justified by the data adduced. Keyvani declares the Safavid régime to be autocratic (correct) and oppressive (largely incorrect as far as the lower classes were concerned). I have pointed out elsewhere that if the Safavid monarchy had *not* been absolutist it would have been the exception rather than the rule in the seventeenth century. Even in Europe, absolute monarchy was the standard form of political organization until the French Revolution. As to oppression of the lower classes, Keyvani is wide of the mark in many instances. He states that "the peasants and nomads continued . . . to live hard lives under the domination of landowners and khans" (p. 251). Granted their lives were hard, but Chardin (quoted in Ann Lambton, *Landlord and Peasant in Persia*, p. 127) compared their condition favorably with that of peasants in the more fertile parts of Europe. Keyvani asserts that the caravanserais were "primarily to serve royal and official needs" (p. 221). Tavernier, another French visitor to Iran in the seventeenth century, comments specifically on the democratic character of caravanserais, which, he says, were designed to benefit poorer travelers; in caravanserais supported by benefactions, travelers were provided with food gratis. In rural caravanserais, accommodation was free; in towns, there was a nominal charge. In short, the data amassed by Keyvani are most useful; the conclusions he draws from them must be examined carefully.

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ERVAND ABRAHAMIAN. *Iran between Two Revolutions*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1982. Pp. xiii, 561. Cloth \$45.00, paper \$14.50.

Publications about contemporary Iran have become something of a growth industry with the appearance of some three dozen titles in only four or five years. Few, however, are worthy of merit; of these, Ervand Abrahamian's *Iran Between Two Revolutions* is one of the most important. This book makes three significant contributions: its class analysis will force all of us—Marxist and non-Marxist alike—to re-examine our ideas about Iran's twentieth-century history and

will provide the basis for discussion for some time to come; it gives the best account of the development of the Tudeh party and its social, intellectual, and political bases; and it presents the most detailed account of the Pahlavi period (ca. 1921–78) and its political history. Not only is Abrahamian's analytical framework a major contribution but he supplies much new information, often from sources not readily available.

Abrahamian begins with a brief theoretical introduction and then turns to a longer but still succinct survey of the structure of Iranian society under the Qajars and the nature of their rule. He examines the Constitutional revolution (1905–11) and Iran's disintegration during World War I, and brings the first part of his study to a conclusion with Reza Shah's reign and the establishment of the nation-state. Part 2 focuses on the reign of Muhammad Reza Shah (1941–78), especially social and political developments and the emergence of an Iranian left. The theme of "political underdevelopment" (in contrast to the important economic and social developments under the Pahlavis) in face of an increasingly politicized society continues into part 3, which deals specifically with the revolution and the establishment of the Khomeini government.

Throughout this book Abrahamian examines the social base of politics and rule. In Qajar Iran he identifies the traditional four classes linked "vertically" and each fragmented into a variety of groups, and the whole manipulated by the Qajars: "The Qajar dynasty ruled nineteenth-century Iran with neither the instruments of coercion nor the science of administration, but with the practice of prudent retreats and the art of manipulating all the possible variations in the complex web of communal rivalries." Abrahamian notes that the Qajars and traditional society were undercut by the Western impact and that "horizontal" class consciousness began to develop as a consequence in the Constitutional revolution period, first with the traditional middle class and then with the emergence of a modern intelligentsia.

Iran is led out of the social and political chaos of World War I by Reza Khan—later shah—who established the centralized nation-state based on a standing army, bureaucracy, and Pahlavi patronage. He used a wide-based urban support to crush a variety of traditional, often ethnic, leaders and sought to integrate the population under new symbols of Iran's great pre-Islamic heritage and the anticipated results of modernization. Such policies—and their accompanying social change—allowed for class formation, especially the first generation of industrial workers, and the emergency of the Tudeh. Abrahamian notes the internal contradictions that were not resolved by either of the Pahlavis: "For the new regime, despite impressive institutions, had no

viable class bases, no sound social props, and was thus without firm civilian foundations. The Pahlavi state, in short, was strong inasmuch as it had at its disposal powerful means of coercion. But was weak in that it failed to cement its institutions of coercion into the class structure."

Iran's modern history is a fascinatingly complex one and continues to be understudied. Abrahamian deals with this complexity in a significant way, although each of us in our own specialization may find some aspect less than satisfactory. This reviewer, for example, might question some of Abrahamian's analysis of traditional and nonurban society (here he reflects too much his British sources), or others may disagree with his conclusion that the revolution and its aftermath was brought about by a society that defined itself in terms of class, or that Khomeini may be likened to Lenin. Nevertheless, he gives meaning to modern Iranian history in much the way that E. P. Thompson, a scholar whom Abrahamian greatly admires, admonishes: "If we stop history at a given point, then there are no classes but simply a multitude of individuals with a multitude of experiences. But if we watch these men [and women] over an adequate period of social change, we observe patterns in their relationships, their ideas, and their institutions. Class is defined by men [and women] as they live their own history, and, in the end, this is its only definition."

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AFRICA

DOUGLAS PORCH. *The Conquest of Morocco*. New York: Alfred A. Knopf. 1982. Pp. xii, 335. \$16.95.

Douglas Porch in his work on the French annexation of Morocco states that "The story of the conquest is a story of people, of chaos, villany, glory, misery, violence, greed, avarice, and maladministration. It is not for those who like their history neat." With this in mind, the author presents a rather popularized view of the Moroccan venture for the general reader. While there appears to be no central thesis or new interpretation, the main thrust of the work concerns the French military in general and the figure of Maréchal Lyautey in particular. The political, diplomatic, and social history of the events leading up to the Treaty of Fez, which brought Morocco into the French empire in 1912, are not forgotten, but they are subordinated to the colorful, adventurous story of the French army's role in the acquisition. Porch has subtitled his book, "The bi-

zarre history of France's last great colonial adventure, the long struggle to subdue a medieval kingdom by intrigue and force of arms, 1903–1914." For those who look for the colorful, the exotic, this work will not disappoint. For those who search for new scholarship, there is a great deal to be desired.

A good bit of the work is taken up with the interaction of Europeans with Moroccans prior to the intensive period of penetration after 1900. Porch does a good job in surveying the European's ethnocentrist reaction to a land that they perceived as being barbaric and medieval. The author does state that, regardless of how many Europeans visited Morocco and how many dispatches passed between the embassies and their foreign ministries back home, a real knowledge of Morocco and Moroccan ways was lacking. He argues that the French, especially men like Foreign Minister Théophile Delcassé and George Saint-René Tallandier, France's representative in Morocco, misinterpreted the statements of Moroccans about their ineffectual and corrupt government to mean a welcoming by the elites of a French imperial presence. In this the author is correct because the Maghzen, Morocco's urban educated elite, would work to keep the state's independence intact. European travelers were too often overcome with the exotic nature of Morocco, with tales of its past struggles with the Spain of the *reconquista*, and too ready to prejudice Islamic culture to be objective about what they saw. But Kenneth L. Brown in his *People of Salé: Tradition and Change in a Moroccan City, 1830–1930* (1976) tells us that beneath the surface, unobserved by the Eurocentrist visitor, there was dynamic change—an Islamic urbanism, marked by the concentration of wealth in the hands of the elites, with more dependence on a centralized Moroccan government.

This work is for general libraries rather than for the research scholar. Porch, a professor at the University College of Wales who has written several books on the history of the French army, prefers to chronicle the adventures of the French military in Algeria and Morocco during this period. He is critical of Maréchal Lyautey's efforts and methods, but he limits himself to Lyautey the soldier rather than Lyautey the colonial administrator, the role in which he did perhaps his best work. Porch's style of footnoting is somewhat odd, but he has consulted most of the major secondary works on French expansionism and on Morocco. Possibly a close look at the Terrier Papers in the *Bibliothèque de l'Institut* in Paris would have shown him the role of Auguste Terrier in the acquisition of Morocco, a vital part of the story that is not mentioned by Porch. The author does not claim to break new ground with new archival material. What he does claim is that this book is a bizarre history of France's last colonial venture, and he delivers on his promise with a

readable account of the military's efforts in the acquisition of Morocco.

JAMES J. COOKE
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HAGGAI ERlich. *The Struggle over Eritrea, 1962–1978: War and Revolution in the Horn of Africa*. (Hoover International Studies.) Stanford: Hoover Institution Press of Stanford University. 1983. Pp. xiv, 155. \$9.95.

By early 1978 the various Eritrean liberation movements had taken such advantage of the chaos in Ethiopia after the 1974 revolution that they controlled the whole of Eritrean territory, save only for four or five besieged Ethiopian garrisons. Their goal of an independent Eritrea, reversing the 1951 United Nations decision that had federated the former Italian colony with Ethiopia, seemed to be well within their grasp. A few months later, the Ethiopian army had recaptured the whole province, except for some isolated strongholds in the far north. How, Haggai Erlich asks, was this historic opportunity to achieve an independent Eritrea lost?

He answers basically in terms of the shallowness of the new Eritrean nationalism, compared with the resilience of the Ethiopian nationalism that opposed it. Whereas the Ethiopians revived their ancient sense of nationhood with the slogan "Unity or Death," the Eritreans remained divided into several movements that were split by personal and ideological rivalries, while the most important movements clung to an ideological rigidity that denied them the support of their natural allies among the Arab states of the region. Given a willingness to compromise, put unity before revolution, listen to experienced leaders in exile rather than hotheads in the field, and accept the idea of an Arab (rather than a Marxist) Eritrea as at least a temporary expedient, it might, Erlich implies, have been a very different story. Is he right?

In a sense the answer is much more an Ethiopian than an Eritrean one. Once the Ethiopians had succeeded in mobilizing a population some fifteen times that of Eritrea, it would be a remarkable liberation movement that could stand in their way. In this sense Erlich's question scarcely seems to need much answering. Yet the Eritrean People's Liberation Front (EPLF), especially, *was* (and remains) a remarkable movement. It attained a level of military effectiveness and ideological cohesion unequalled by any African liberation movement and matched only by the Khmer Rouge in Southeast Asia. It did so among a disparate group of peoples, half Moslem, half Christian, lacking any sense of national identity save that produced by alien rule. The longer answer to Erlich's question is, thus, that the ideological

militancy of the EPLF was necessary to make it an effective force in the first place: a compromising movement led by old men in exile could not have induced young men to die in Eritrea, while a pan-Arabic movement would have intensified the internal divisions that Marxism helped (without complete success) to bridge. The Eritreans' failings were the necessary corollary of their achievements.

At many points in this book the author's Israeli nationality shows through, not simply in the telling reference to the Palestine Liberation Organization "as always in the service of Moscow" (p. 116), but in preventing him from speaking to Eritreans or their main external backers. He has conscientiously trawled the available sources, and the account he gives is for the most part both accurate and fair. To this extent, it is a useful book. But a sense of what it is all about escapes him.

CHRISTOPHER CLAPHAM
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JOAN VINCENT. *Teso in Transformation: The Political Economy of Peasant and Class in Eastern Africa*. Berkeley and Los Angeles: University of California Press. 1982. Pp. viii, 307. \$28.50.

Joan Vincent's study of the transformation of Teso society in eastern Uganda during the early years of colonial penetration is a useful addition to a growing number of historically based studies written by anthropologists as well as to recent works by historians on the incorporation of African societies into an expanding worldwide capitalist economy. Vincent's extensive use of historical materials and methods to delineate social and economic changes among the Teso represents a welcome advance over the more perfunctory use of historical sources by earlier anthropologists. Her book deserves comparison with the recent historical studies by anthropologists T. O. Beidelman, R. G. Willis, and most particularly I. Karp, whose penetrating analysis of social change among the related Teso of Kenya during the colonial era, *Fields of Change Among the Iteso of Kenya* (1978), is curiously absent from Vincent's citations.

The strength of Vincent's study, in comparison with Karp's, is its extensive detail on the expansion of administrative structures and personnel, first Ganda agents and then British colonial officers and Teso chiefs, the establishment of Christian missions, and the creation of new economic relations associated primarily with the introduction of cotton growing and the demands for forced and voluntary labor. While much of her data on the establishment of British colonial rule comes from secondary sources, Vincent has mined the appropriate archives in London and Uganda to add new material to the

picture of colonial penetration. But her efforts at the "new social history" in chapter 6, in which she employs census and tax records to reconstruct social patterns in Teso society, are somewhat flawed. She fails to address fully the methodological problems related to the use of such data and makes some questionable interpretations. For example, she suggests that the more than twenty thousand men listed as employed in bridgework, road building, and factory construction in 1921 represent a rural proletariat, when these occupations were almost certainly temporary labor obligations imposed by colonial administrators on the Teso peasantry. The analysis does, however, lead to some interesting conclusions about the heterogeneous composition of Teso society during the early decades of this century.

The central weakness of the study, again in comparison with Karp's, is Vincent's failure to examine the impact of colonial rule in terms of the specific values and structures of Teso society. In her effort to place the Teso into a wider historical and intellectual context, she seems to have lost sight of the Teso as a people. Despite her claim that the book examines Teso society from the bottom up, Vincent focuses far more on the administrators, commercial interests, and missionaries, whose activities rechanneled Teso lives, than on the responses and initiatives of Teso peasants.

This failure to examine change from within Teso society is perhaps the result of her reliance on archival sources and the concentration of her field work on Teso working within an urban trading center, on chiefs, and on a marginal group of Bakenyi fisherman. She apparently did not conduct extensive research among the Teso peasantry, who are allegedly the central focus of her study. Yet, it may also reflect her effort to squeeze the Teso experience into externally defined categories of change—the making of a peasantry or the making of a rural proletariat—derived from English social historians, such as E. P. Thompson and Eric Hobsbawm, and from the dependency theory of Gundar Franke, Samir Amin, and others. While such categories may well be appropriate, concentrating on them has caused the author to lose sight of the more subtle, but no less profound changes in kinship, residence, property, and rural patterns that Karp so clearly identifies among the Kenya Teso. Vincent is certainly justified in attacking cultural anthropologists for "attaching no theoretical or substantive importance to the development of capitalism," (p. 7), and she is to be praised for having attempted to correct this bias in her own study. One must ask, however, whether the application of externally defined models without reference to particular cultural forms is not equally distorting.

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ASIA AND THE EAST

GEORGE ALEXANDER LENSEN. *Balance of Intrigue: International Rivalry in Korea and Manchuria, 1884–1899*. In two volumes. Foreword by JOHN J. STEPHAN. Tallahassee: University Presses of Florida, for Florida State University. 1982. Pp. xviii, 476; vi, 477–984. \$62.50 the set.

This is the last work by George Alexander Lensen, a most distinguished scholar of Russian–East Asian relations, whose untimely death occurred just three years ago as the result of an accident. The book represents a painstakingly detailed narration of international power rivalries involving Korea, Manchuria, and the Shantung Peninsula during the two decades 1880–1900, and it makes the fullest possible use of multiarchival materials, taken from Russian, British, French, German, Chinese, Japanese, American, and Korean sources. Lensen's meticulous attention to detail and his untiring pursuit of original sources make these two volumes a truly definitive study, but it also creates problems, of which, clearly, the two most important are style and analysis.

As to style, this perhaps can best be illustrated by quoting a paragraph from the chapter on the Japanese peace terms at the conclusion of the Sino-Japanese war in 1895 and the concern of the great powers with those terms: "Hitrovo's statement, Mutsu noted to Nishi, was at variance with his previous official declaration that so long as the independence of Korea was left unimpaired, Russia would not object to other demands by Japan. 'You will always bear that declaration in mind in case of dealing with the Russian Government,' Mutsu instructed Nishi. He related that the Austrian charge d'affaires had confidentially told Hayashi that day that the secretary of the Russian legation, Grigorii Aleksandrovich Vollen, had said that 'Japan shall not get an inch of territory as the condition of peace.' 'That day too Yokohama newspapers had published an extract from a Russian newspaper to the effect that 'if Japan wants to obtain territory on the mainland, she must be prepared to face Russia as her enemy.' Matsu asked Nishi to check whether such a statement had indeed appeared in the Russian press" (p. 259).

The two volumes contain 854 pages, and every one of them is chock-full of the kind of detail quoted above. Although the work thus represents an unsurpassed reference book, it is debatable whether the question of allowing sedan chairs into the palace grounds of the Korean king in 1887 and Yuan Shih-k'ai's arrogance in this matter (chap. 4) or all the innumerable and somewhat gory details of Kim Ok-kyun's murder in Shanghai (p. 113)—he had a Chinese novel in his hands and was reclining in a wicker chair when attacked—really serve much of a purpose.

More importantly, there is the matter of analysis. Lensen concludes that the policy of Russia in East Asia during the period under consideration was not as aggressive as previously depicted, especially by British writers. He states that Russia was a weak empire with no grand scheme, and its actions in Korea were the result of on-the-spot decisions. Russian policy, characterized by caution and restraint, derived from awareness of Russia's geopolitical weakness in the Far East and its insufficient naval power in the Pacific. Lensen argues that Russia exercised remarkable restraint with Korea and agreed to have Russian army officers train the Korean army only because Russia feared that if it did not do so, Japan would. Russia, furthermore, did not want to intimidate the Korean government, but was lured by the intrigues of Korean officials and its own representatives in Korea into questionable actions. Russia sincerely desired peace and improved Russo-Japanese relations, especially after the seizure of the Liaotung Peninsula in 1898, as reflected in the Nishi-Rosen agreement. Lensen's points may be true, but one still has to inquire into the underlying nature of Russian policies, both prior and subsequent to that rather arbitrary chronological framework chosen by the author. Did not Russia acquire the vast lands of the Maritime Province (Treaty of Peking, 1860) and build the great naval base of Vladivostok ("Master of the East") as the result of good, old-fashioned imperialism? Were the actions of Russia in Manchuria in 1900 and in Korea (Bezobrazov) not likewise the result of imperialist ambitions? The word "imperialism" does not appear even once in either of the two volumes, thus indicating, in my opinion, a certain lack of conceptualization. Would it not be better to describe these two decades as mere interludes, retreats one might say, chosen out of political, economic, and naval weakness, when Russian attention was more closely riveted to the West? Is the pendulum theory of Russian imperialism, shifting from East to West and back again, not a better indicator of general Russian imperialist and expansionist policies in the Far East?

My criticism of Lensen's work in no way should detract from the immense labor it represents and the highest quality of its research. It is a truly monumental study, but its impact is somewhat vitiated by an absence of critical analysis. Nevertheless, it is a work of such quality that it is unlikely that it will be superseded for many years to come. The world of scholarship is made much poorer by Lensen's death. The two volumes, as one would expect from such a master of archival research, include extensive notes, a first-rate bibliography, and a detailed index.

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JAMES L. MCCLAIN. *Kanazawa: A Seventeenth-Century Japanese Castle Town*. (Yale Historical Publications, Miscellany, number 128.) New Haven: Yale University Press. 1982. Pp. xii, 209. \$20.00.

In 1700 Japan contained what was probably the largest city in the world (Edo; present-day Tokyo) and had a greater proportion of its population living in cities than any other region in the world except possibly England-Wales or the Netherlands. Despite these facts, Japanese urban history has to date received only scant attention by Japanese and Western scholars alike. Here, for the first time in English, is the history of one of Japan's castle towns during the century of its most rapid growth, from the late sixteenth to the late seventeenth centuries. In 1583 Kanazawa was selected as the administrative and military center for Japan's largest domain. Construction of the castle that was to form the core of the city began only in 1592, but by 1697 the commoner population numbered 68,636—a figure that would indicate a total population of about twice that size when samurai, priests, and outcasts are included.

James L. McClain's study focuses on this century of growth, which he divides into three periods: "formation," "consolidation," and "closure." Within each period, he discusses the use of space and urban planning, economic growth, the political system and administration of the city, and culture. His method, basically descriptive, uses primarily the official records of the city, especially various regulations. The study is rich in illustrative detail; examples of subjects treated include the difficulties in regulating prostitution, the role of the outcasts in the city, and the story of the samurai who abandoned his status to become an official producer and purveyor of bean curd to the daimyo. Nonspecialists will find the book easy to follow because McClain so nicely places the developments in Kanazawa into the context of national events and explains Japanese terms to the reader.

McClain's study confirms the generalizations made about Japan based on studies of developments at the center (the Edo and Kyoto-Osaka regions) and in other outlying areas, particularly in western and central Japan. Although McClain substantiates and adds breadth and depth to what is already known, he does not excite us with new insight into this extraordinary century of city building. One reason may be that he has tended to remain within the confines of the interests of Japanese scholars, who, McClain states, tend to write general surveys or limit themselves to "a single aspect of urbanization, typically urban planning or commercial development" (p. 2).

McClain's stated purpose is to "demonstrate that urban commoners can play a historically significant

role, even within the context of a highly statist society" (p. 154). With this assertion, he takes issue with Japanese historians who "argue that the early castle towns were chiefly the result of autocratic, daimyo-centered planning" (p. 2). Unfortunately, this is a nonissue for most Western scholars of Japan, who already hold the views McClain reaches in his conclusion.

One hopes that with this study as a start, McClain will go on to try to answer some of the larger questions about seventeenth-century urban growth. What underlay this unprecedented phenomenon that created cities all over the face of Japan in just a century? Why did this urbanization end, almost as suddenly as it began? What enabled Japan to urbanize so extensively in its premodern period? These are extremely difficult questions, but ones for which we would like to have answers. McClain's journeyman-like work shows how far we have come but how much still remains to be done.

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TAKAFUSA NAKAMURA. *The Postwar Japanese Economy: Its Development and Structure*. Translated by JACQUELINE KAMINSKI. Tokyo: University of Tokyo Press; distributed by Columbia University Press, New York. 1981. Pp. xiv, 277. \$20.00.

Takafusa Nakamura, of the University of Tokyo, is a former director general of the Economic Research Institute of the Economic Planning Agency, an official body affiliated loosely with the Ministry of Finance. This volume, one of a trilogy of book-length studies by Nakamura, covers the economic history of his country since the Meiji Restoration of 1868. Its original title is *Nihon Keizai: Sono Seichō to Kōzō* (1980); it is the first volume of the trilogy to appear in English. A second English-language volume, basically the first of the Nakamura trilogy, has also been revised for an English version. It is *Economic Growth in Prewar Japan* (1983). The intermediate volume, as yet available only in Japanese, is *Nihon no Keizai Tōsei* (1975). It deals with the wartime economy, particularly the system of economic controls.

It is, I hope, not unfair to Nakamura to call his work an "establishment" view of a century and a quarter of Japanese economic development. His viewpoint distinguishes itself from both the essentially Marxist "left deviation" that dominates much of Japanese economic history and the "right deviation," a school, consisting largely of foreigners, that ascribes a larger role to the competitive market and a smaller one to bureaucracies both public and private. The demand for and interest in Nakamura's work springs both from the second

Japanese miracle—or, if you prefer, the second flowering of the "fragile blossom"—after the 1945 surrender and also from the recent availability of a wealth of information on Long-Term Economic Statistics of Japan (LTES) developed primarily at Hitotsubashi University.

The Postwar Japanese Economy is more obviously the product of a statistician than of either an ordinary economist or an ordinary historian. Much of the text is an extended gloss on, and accompaniment to, the tables devised or collected by the author with admirable industry and ingenuity. The style, at least in translation, is decidedly low-key if not pedestrian. The non-Japanese reader unfamiliar with recent Japanese economic history at a higher level than, say, the historical chapters in Hugh Patrick and Henry Rosovsky's study of *Asia's New Giant* (1976) is likely to miss the economic forest for the statistical trees.

Nakamura plays down both controversy and individuality. I say it boldly and baldly: economic history, like economics and like history, should be written as if controversy mattered and also as if people mattered. By this I mean that the subject should not be presented as cut-and-dried where it is not and that the reader be kept informed as to the existence of controversy as well as to the evidence that supports the writer's conclusions. Nakamura follows an upper-class Japanese preference for polite blandness, so that his uninformed readers might well imagine that 98 percent of Japanese economic historians agree with him 98 percent of the time. This they decidedly do not; there are Marxists in them there hills and on them there campuses!

As for "people mattering," we are, I think, entitled not indeed to full-fledged doctrinal history but to some desultory acquaintance with key individuals who humanize and personify the impersonal economic trends and forces of the statistical tables. In America, these people might include Edison and Ford, Rockefeller and Vanderbilt, FDR, JFK, and Ronald Reagan. On the postwar Japanese scene, consider: Joseph Dodge along with General Douglas MacArthur of the Allied Occupation, at least three prime ministers who represent definite ideas (Yoshida Shigeru, Ikeda Hayato, and Tanaka Kakuei), Ishibashi Tanzan of *Tōyō Keizai Shimpō* as the voice of (inflationary) Japanese ultra-Keynesianism, Okita Saburo and Shimomura Osamu as precursors of the economic miracle, Tsuru Shigeto as the voice of caution and advocate of tighter planning, and Sahashi Shigeru of MITI as personifying administrative guidance and "Japan, Inc." Most of these people—and others of equal standing—do not even figure in Nakamura's index!

In economic analysis, Nakamura's greatest strength in his mastery of the high-level input-output techniques of Wassily Leontief and their

application to the Japanese scene. Somewhat offsetting this virtuosity is his occasional use of what Don Patinkin calls the "invalid dichotomy" between the real and monetary aspects of an economy. His "real" discussion tends to ignore "monetary" interactions, and vice versa. Sometimes money matters not at all; then again, sometimes it matters almost exclusively.

To summarize, Nakamura has given us an indispensable reference book. Those with adequate backgrounds will also find in these pages and appendixes valuable refresher minicourses for gaps in their training and points they have forgotten. He has *not* proposed to write an introductory text for foreign undergraduates, either historians or economists, in recent Japanese economic history. This, unfortunately, is the way this reviewer tried to use it; the disappointment in his review should, I fear, have been directed more against himself and less against our author.

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GIUSEPPE TUCCI. *The Religions of Tibet*. Translated by GEOFFREY SAMUEL. Berkeley and Los Angeles: University of California Press. 1980. Pp. xii, 340. \$19.95.

This book by Giuseppe Tucci, the internationally renowned Tibetologist, is a scholarly study of the religions of Tibet: Buddhism, the nameless "folk religion," and the system called *Bon*. The history of the spread of Buddhism in Tibet is divided in the indigenous tradition into the "early" and "later" stages. The first chapter of the book surveys the significant events of the early spread, which ended with the persecution of Buddhism in the ninth century, and the second reviews those of the later spread, beginning with the revival of Buddhism and the founding of great monasteries in the eleventh century. Chapter 3 deals with the general characteristics of "Lamaism" and the emergence of the major schools of Tibetan Buddhism, particularly the rNying ma pa, Sa skya pa, bKa' brgyud pa, and dGe lugs pa. Chapter 4 examines the doctrines held, both in common and in particular, by these schools, as well as the substantialism of the Jo nang pa and the quietism of the Zhi byed pa. The fifth chapter deals with the organization of the monastic community, the administration of the monastery and its property, and the religious calendar with its various festivals. Chapter 6 is devoted to the "folk religion," replete with its beliefs in benevolent and malevolent numina. Various apotropaic rituals intended to protect the individual, the family, the house, and other property are discussed in detail. This chapter shows clearly the contradiction between the intellectual preoccupation with Buddhist epistemology and

ontology on the monastic level and the emotional concern with the existence of demonic powers and the vulnerability of the "soul" (*bla*) on the lay level. The final chapter deals with the *Bon* religion that predated Buddhism in Tibet. This chapter explores the religious milieu of the ancient monarchy and then examines the way in which *Bon* evolved over the centuries in competition with, and later in imitation of, Buddhism. An eight-page chronological table listing significant dates and events in Tibetan history is given at the end of the book.

Only a few flaws in the historiography were noted. Tucci says that the territory of Tibet was divided in the thirteenth century "into thirteen districts (*chol kha*)" (p. 27). The number should be three. In the lengthy discussion on the name and symbolism of the king named *Gri gum*, "Killed by the Dagger" (pp. 223–27), the author curiously does not mention that this king's name is spelled *Dri gum*, "Killed by Pollution," in the ancient Tun-huang documents. The chronological table says for the year 1182 that Sa skya pandita was recognized as a "representative of the Great Mongolian Empire in Tibet" by Ögedei (p. 251). Ögedei, son and successor of Chinggis Khan as ruler of the Mongols, died in 1241. The sources date Sa skya pandita's first encounter with the Mongols in 1244; therefore he could hardly have been recognized as a representative by Ögedei. For the year 1244, the text states Sa skya pandita met with "the Mongolian king Godan" (p. 251). According to traditional sources, this meeting did not take place until 1247. Moreover, even though Godan, the second son of Ögedei, is called "king" (*rgyal po*) in Tibetan accounts, he never was a "Mongolian king," that is, a Khan or Khaghan. The book refers to the presence in the year 1751 of "the two Chinese residents (*amban*)" (p. 254). It should be made clear that the officials in Tibet who held the Manchu title *amban* were always Manchus or Mongols, never Chinese. Finally, the date for the "occupation and annexation of Tibet by the People's Republic of China" is given as 1949 (p. 256). The Chinese invaded in October 1950, and the occupation and annexation of Tibet took place the following year.

This book synthesizes a great deal of published information and is illustrated with delightful line drawings by Lama Namkhai Norbu of Naples. It is an invaluable contribution due to its wealth of new data and insights based on the author's years of research in the primary sources and elucidated by his personal observations during several expeditions into Tibet in the 1930s and 1940s. In short, this essential reference work belongs in the library of every serious scholar in the field of Tibetan religions.

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ALFRED W. MCCOY and ED. C. DE JESUS, editors. *Philippine Social History: Global Trade and Local Transformations*. Honolulu: University Press of Hawaii or Ateneo de Manila University Press, Quezon City, Manila. 1982. Pp. vi, 479. \$9.50.

The coeditors of this excellent work, Alfred W. McCoy and Ed. C. de Jesus, have dedicated it to their late teachers: Horacio de la Costa and Harry J. Benda. Those distinguished pioneers would be proud, for here are fifteen essays by thirteen scholars, providing for the first time a series of regional and local pictures of continuities between the nineteenth and twentieth centuries, without neglect of discontinuities. Ways of conceiving the Philippine nation become clearer and sounder for having to pass the test of understanding Philippine regions, economies, and societies.

The contributors are Filipino, Australian, and American. Vocationally, they include historians with different methods and subfields, two anthropologists, and a geographer. Their collective achievement, a socioeconomic history of an intensity unparalleled for other parts of Southeast Asia, is based on the richness of documentation in the Philippines, relative freedom of access to sources, and their own exuberant assiduity in pursuit of historical truth.

The subtitle and the introductory essay suggest binding themes of a neo-Marxist sort. In order to develop such themes, historians must inevitably explore the role of "elites" in society. But the problem of defining "elites" in the Philippines is a difficult one, on which a baker's dozen of independent thinkers will not agree, including the authors of these essays. De Jesus takes us into the vexed problem with a study of the consolidation of power in Cagayan by "the traditional *indio* elite which had ruled as *datus* before the [Spanish] conquest" (p. 33). Dennis Roth, looking at uprisings on church lands in the Tagalog region, is able to avoid the term "elite" in describing those minisocieties; and William Scott, considering the Spanish occupation of the Cordillera, sidesteps it effectively with regard to a relatively rudimentary social structure. Several others, however, such as Jeremy Beckett, writing on the *datus* of Magindanao, struggle with problems of upward and downward mobility and presumptive justice or injustice. Amid essays that include references to "multi-ethnic . . . urban aristocracy" (p. 278), "literati leadership" of unions (p. 333), and "super-*principalia*" (p. 391), it is disarming to be reminded of the late Frank Lynch's simple distinction between "big people" and "little people" (p. 236). Disarming it may be; but one perhaps needs the definitional equivalent of a *bolo* to clear a path. Help comes from Brian Fegan (p. 125 n. 24), who makes a contextual distinction between cultural status and political power, and from Milagros Guer-

rero (p. 179 n. 1) in her provocative essay on "The Provincial and Municipal Elites of Luzon during the Revolution, 1898-1902."

At issue, of course, in questions regarding "elites" is, Who has what kinds of power and what sorts of initiative? McCoy, in his introduction, attempts to refer these matters and others to the global interpretations of Immanuel Wallerstein and André Gunder Frank, particularly the former. He declares that a major aim of the book is to meet the need for understanding "the integration of Asia, Africa, and Latin America into this world capitalist system as 'periphery' or 'satellite' states" (p. 5). Such an aim, of course, helps constrain against possible provincialism and antiquarianism in the essays, while introducing the possibilities of overinflated globalism and overdetermined teleology. The individual authors, for the most part, walk the wire nicely, with balance, discipline, and style.

Marshall MacLennan's and Fegan's essays are complementary, and both apply Hans Bobek's theory of rent capitalism to human ecology in Nueva Ecija and to the sociopolitical dynamics of a barrio in Bulacan. Fegan shows that from the peasant unions of the 1920s through the defeat of the Huks in the 1950s there existed a homogeneous class of tenant farmers. But the closing of the agricultural frontier has since then fragmented village social structure with "bureaucratic involution," "services involution," and a growing class of landless laborers.

Norman Owen's essay on abaca in Bikol stresses the sense in which the area was "doubly colonized" by the West and Manila. While evoking sympathy for those wholly dependent on the economic cycles of England and the United States, he concludes on a note of pride on behalf of those not motivated by "development" or collective destiny but by local values, "traditional culture," and personal opportunity. Some of Owen's language conforms to the Wallerstein outlook, but much of his analysis tends to support assertions of regional cultural integrity.

With regard to the presumptive dynamics of global capitalism, other authors develop lines of analysis that are abstemious, resistant, or deviant. Bruce Cruikshank uses Samar to suggest that "for most 19th century Filipinos, the norm was not the hacienda but the scattered subsistence settlement only partly committed to commercialized agriculture" (pp. 239-240). Ronald Edgerton shows the diverse economies of the Bukidnon plateau, where the evolution from slash-and-burn to plow agriculture occurred at the same time as pineapple growing and cattle ranching developed on leased land.

Michael Cullinane pictures Cebu, convincingly, as an autonomous entrepôt dealing directly with centers of the global economy. Export agriculture in this instance promoted regional autonomy, as opposed to national integration. If part of a mercantil-

ist world system, Cebu had little to do with forming a nation or a state. James Warren's study of the Sulu sultanate shows clearly that in this case external trade, slave raiding, and state formation were "inter-related themes." Nonetheless, one may observe, Spanish imperialism found this "pirate and slave state" offensive and wiped it out. What is proven? Perhaps Warren's findings support Schumpeter's reasoning that the sociopolitical bases of capitalism and imperialism are dissimilar, just as Cullinane's implicitly support Schumpeter in dissociating mercantilism from the formation of states.

Among the scholarly steeds breaking out of the Wallerstein corral is the coeditor, de Jesus. His concluding essay summarizes nicely some of the questions touched on by several others and restates the importance, from the Filipino point of view, of the local-national axis and of "political alliances built on a network of patron-client relations and reinforced by marital connections and compadrazgo ties." Since 1972 there has been a major break with the past—"a determined attempt by one faction among the elite to establish the supremacy of the centre" (p. 448).

McCoy's own contribution is a sterling essay entitled "A Queen Dies Slowly: The Rise and Decline of Iloilo City." Its argument reduces the importance of dates like 1898 and 1946 by focusing on the continuity of social, economic, and political processes. The discontinuities in this instance are the rise of the sugar industry (displacing native textile production) and the decline of the same (replaced by nothing). McCoy chooses to describe Nicholas Loney, who promoted sugar in the mid-nineteenth century, as the perpetrator of the downfall of Iloilo's prosperity. By so doing he tries to draw blame away from José Maria Nava, whose career as union leader spanned the years of Iloilo's decline a century later. Because McCoy personalizes the city as a "queen," he requires himself to look for an "assassin." Wallerstein's theory appears to suggest that the villain must be a foreign capitalist. Loney gets the murder charge, and Nava is acquitted. My doubts aside, both density of research and trenchancy of expression make this a memorable essay.

This book is an achievement of the first order in the historiography of Southeast Asia, the Pacific, and the filling in of global frontiers. The scholars, aided by excellent maps, convey a vivid appreciation for Philippine regions and social enclaves. They require and enable us to understand socioeconomic change across political regimes. Borrowing usefully from anthropology, geography, and economics, the authors open new frontiers of cognitive terrain for careful understanding.

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MICHAEL STENSON. *Class, Race, and Colonialism in West Malaysia: The Indian Case*. Vancouver: University of British Columbia Press. 1980. Pp. xii, 234.

Michael Stenson died tragically young in a Rangoon hotel of a cerebral fever. All his professional life he had scorned the sort of historian who saw Southeast Asia only from the files of the Colonial Office Library. In 1965 he won a Commonwealth Scholarship to pursue doctoral studies at the University of Malaya in Kuala Lumpur, and he began working as an anthropologist might. He lived with and learned the language of the people whose past he was studying, he accommodated to their customs and made lasting friendships, and he wrote *Industrial Conflict in Malaya: Prelude to the Communist Revolt of 1948* (1970).

This posthumously published *Class, Race, and Colonialism in West Malaysia* is subsidized from the Michael Stenson Memorial Fund. Those administering the fund made the wise decision that the book's publication comprises the most fitting of memorials to him. It is a continuation of his earlier work and a refinement of it. In many ways it can be read as Stenson's intellectual autobiography, for it adumbrates his journey in search of understanding Malaysian realities past and present.

In his earlier book Stenson for the most part accepted the conventional plural society model for analyzing Malaya's multiracial society, but in the present book under review he has moved to a more ideologically explicit stance. He argues that the concept of a plural society made up of separate communities interacting mainly in the marketplace is of insufficient explanatory value. Instead he now seeks to examine the political economy of West Malaysia as a whole, particularly the ways in which it has been affected by the evolution of international capitalism. In adopting this approach he confesses to have been strongly influenced by critiques of skewed development in the Third World by such writers as G. L. Beckford, G. Kay, E. L. Wheelwright, and James Puthucherry.

In this second book Stenson argues that within and between the communal interstices of West Malaysian society can be found the conceptually more sophisticated and analytically more useful realities of class division and class solidarity. He acknowledges that it would be nonsensical to set aside entirely the fact of racial difference—indeed he continues to focus particularly on the Indian community that he knows best—but his aim is to use it as a prism through which to view the complexities of the socioeconomic whole.

His argument is carefully crafted and clearly presented. He uses an ideological framework to shape his account of colonial and postindependence developments in peninsular Malaysia, but he is far

too skilled a historian to jettison particularities, precision, or detail. It is an intellectually stimulating and satisfying book to read but in the final analysis a depressing one, for he argues persuasively that those holding power in independent Malaysia are no more eager to free their country from its neo-colonial economic status than were their timidly nationalistic precursors.

Hail and farewell Michael.

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R. DE BRUIN. *Islam en nationalisme in door Japan bezet Indonesië, 1942–1945* [Islam and Nationalism in Japanese-Occupied Indonesia, 1942–45]. (Rijksinstituut voor Oorlogsdocumentatie, Cahiers over Nederland en de Tweede Wereldoorlog, number 3.) The Hague: Staatsuitgeverij. 1982. Pp. 104.

This little book by R. de Bruin, for many years associated with the Indies Section of the National Institute for [Second World] War Documentation in Amsterdam, is organized around the idea that by comparing Japanese policies toward Islam in three rather different areas of the occupied Netherlands East Indies, some important questions may be answered. Were, for example, Japanese approaches to Islam very different from those of the Dutch, and if so why? Were Muslims helped or hindered by these policies? What relationship developed between Muslims and the so-called secular nationalists (most of whom, it should be pointed out, were also Muslims) during the occupation, and how did this relationship influence events once Indonesia had become independent? As the preface of this work points out, these are complex questions that have received insufficient attention, especially in the Netherlands, where interest has focused primarily on the fall of colonial rule and the fate of Dutch citizens in that process. From this point of view one is especially grateful that de Bruin, who possesses unusual access to and knowledge of archival sources, has turned his pen in this direction.

The chief conclusion of the study seems to be that the Japanese handled Islam with greater flexibility than the Dutch and that they thereby laid the groundwork for both Muslim cooperation with secular nationalists and, in some instances and regions, continued striving for a Muslim state. (I say "seems" because, in expression as well as emphasis, the Dutch-language conclusion and the English summary differ somewhat.) Although there is nothing wrong with this idea as it stands, it does not take us much beyond the judgment reached twenty-five years ago in Harry J. Benda's classic *Crescent and the Rising Sun*, though the scope of that work was

limited to Java. Furthermore, to my way of thinking there are several flaws in de Bruin's formulation. The first is that it is problematic to argue that the Japanese were more flexible than the Dutch because of the Japanese culture's basis in Shintoism, which, like Islam, did not distinguish clearly between religious and political spheres of behavior. De Bruin's own analysis shows rather clearly that in at least two of the three regions surveyed the Japanese made precisely that sort of distinction, specifically curbing Muslim political power. Any telling of the story, including de Bruin's, suggests very strongly that the Japanese everywhere in Indonesia preferred a secular state to succeed them.

The second difficulty is that the principal causative factor in events remains unclear at the end of this work: one is uncertain whether Japanese policy (sometimes uniform, sometimes not) or local conditions should be seen as paramount. The issue is complex, of course, but it ought to be faced squarely rather than left in the rather foggy shape it is here. A third and much less consequential flaw is that the book's mechanics invite criticism. Particularly in view of the author's proximity to archival treasures, the unevenness in citation (and use?) of sources is disconcerting. For Aceh, de Bruin relies almost totally on Piekaar's 1949 publication; for the Outer Islands, he is dependent on newspapers. Finally, there is an incomprehensible retention of the colonial spelling of Indonesian words and even place-names.

None of these problems entirely obviates the usefulness of this monograph, which, like any academic investigation, asks as many questions as it sets out to answer. But they do raise the hope that the author will not abandon the topic at this juncture but devote his talents to a genuinely eight-cylinder, full-horsepower piece of research. For whatever the case in the automotive world, such efforts are far from outdated or unwelcome in the realm of Indonesian history.

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HARRY MORTON. *The Whale's Wake*. Honolulu: University of Hawaii Press. 1982. Pp. 396. \$32.50.

Whaling in New Zealand waters was a branch of a world-wide industry and served as an economic and social preface to settlement and annexation in 1840. From 1792 British vessels predominated for forty years, establishing the links of informal empire with New South Wales and setting a pattern of shore stations that held off the challenge of American whaling from 1830 and a smaller but well-subsidized effort by the French. The very success of the immense slaughter nearly wiped out the right whale

and reduced sperm whale catches to an uneconomic level. New Zealand ports remained important to Pacific whalers after the 1840s, but the first staple of New Zealand's early trade had passed its peak. The whalers who settled moved into agriculture, leaving a rich historical legacy that closely influenced British political intervention and made its mark on the social relations between pakeha and Maori.

The merit of Harry Morton's detailed study is to exploit a wide range of source materials, including the logs of more whaling voyages than were available to his predecessors, such as Robert McNab, and to focus on the industry in one geographic location in order to establish a measure of its impact. Some six chapters cover the distribution of whale stocks, techniques of capture and processing, whalers, vessels, and the use made of a highly substitutable product in the markets of Europe. One misses a consolidated account of quantities imported from the South Pacific and New Zealand in particular, although the problems of attributing origins in the customs records are understandably formidable. A price series might have been attempted for New South Wales and London to test the notion that exhaustion rather than other products explains the decline of the industry in New Zealand waters. No doubt this would have entailed a description of the Pacific whale fishery as a whole, and I hope the author will give us this at some later date.

The remaining sixteen chapters deal with the sealing phase of the hunt for mammals in Australian waters, which established colonial shipbuilding from 1793 and accumulated a store of local knowledge that gave British whalers a lead over rivals. Historians will welcome most the analysis of the effects of Maori recruitment by whalers and the results for Maori trade, warfare, housing, and patterns of intermarriage that the presence of whalers entailed. There is an excellent section on agricultural adaptation and the development of shore markets, the consequences of alien notions of contract for race relations, and the inevitable intervention by naval officers and the courts of New South Wales as incidents on the whaling and missionary frontier accumulated. The case for regarding whaling as a prime cause for extended jurisdiction is overdrawn, however, as I doubt whether in demographic terms the few hundred or so whaling crews, deserters, and occasional settlers who retired from the sea and turned to farming counted more than missionaries or timber and flax traders, although their reputation for lawlessness gave weight to the advocates of British intervention. In the long run their impact on Maori society served to stimulate a remarkable adaptation to the needs of external trade and local markets in produce and land. The competition with American and French whal-

ing, despite the overregulation that Morton contrasts with American whaling efficiency, strengthened the tendency to regard New Zealand as a subimperial dependency of New South Wales.

The general reader will also find much of interest in this entertaining book, which is occasionally garrulous but never dull; historians of the Pacific and New Zealand will use it with profit. The illustrations are generous and well chosen, and the presentation and format are a credit to John McIndoe of Dunedin.

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GLYNN BARRATT. *Russophobia in New Zealand, 1838–1908*. Palmerston North, New Zealand: Dunmore Press. 1981. Pp. 180. NZ \$21.95.

This book by Glynn Barratt begins by showing how tsarist Russia's alleged expansionist ambitions in the Pacific were exploited in the 1830s by promoters of British annexation of New Zealand. Then in the infant colony, isolated by thousands of miles of ocean from the naval bases of mother Britain, fear of the Russians grew to a high point in 1854–55 with news of the outbreak of the Crimean War and rumors of predatory Russian warships in the Pacific. After that war New Zealand's internal problems with rebellious Maoris overshadowed the Russian threat, but it was kept alive in the 1860s by politicians using it to urge maintenance of British defense spending in the colony. Aided by the presence of Polish and Jewish refugees from Russia, fear of the Russians reemerged with Anglo-Russian war scares in 1871, 1878, and 1885. This anxiety contributed to New Zealand's small efforts to build up its own defense capability and its willingness to support financially the maintenance of Britain's protective naval shield. The book ends with the submersion of the Russian bogey by a greater fear of Japan arising from that new Pacific power's dramatic victories over Russia in 1904–05.

The book is most valuable for its contribution to nineteenth-century New Zealand defense history, a subject largely ignored by historians. In that achievement, however, there is also a problem. Although the author acknowledges that fear of Russia was accompanied and at times overshadowed by alarms about French and German incursions into the Pacific, his concentration on Russophobia underplays the extent to which those other threats from powers that actually annexed Pacific islands aroused more concern in New Zealand. For example, France's bid for control of the New Hebrides Islands in 1886 aroused a far greater public agitation in New Zealand than any Russian scare.

Nevertheless, Barratt's book is a scholarly and thorough analysis of New Zealand's long-lasting and at times intense Russophobia. It uses a wide range of primary sources, especially newspapers. And it utilizes relevant Russian literature, which reveals no evidence of any Russian plans to attack New Zealand. At times the evidence is strained, as in the weight placed on one 1831 *Hobart Town Courier* editorial in the discussion of the use of the Russian bogey in promoting the annexation of New Zealand. In this case fears of French intervention were much more important. Barratt's use of secondary sources also has a significant gap. He places New Zealand's fears of Russia into a wider Australasian context, but he ignores relevant studies of Australian defense history published since 1970. These include Geoffrey Serle's history of Victoria in the 1880s, *The Rush to be Rich*; D. H. Johnson's study of defense in colonial Queensland, *Volunteers at Heart*; and Neville Meaney's history of early twentieth-century Australian defense policy, *The Search for Security in the Pacific*.

Overall, however, Barratt has produced a valuable, competently written study of a neglected subject, which also has a modern echo in present-day Russophobia in New Zealand.

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UNITED STATES

DAVID R. GOLDFIELD. *Cotton Fields and Skyscrapers: Southern City and Region, 1607–1980*. Baton Rouge: Louisiana State University Press. 1982. Pp. xiv, 232.

"It is time to place the magnolias on Main Street and to develop an urban view of southern history," David R. Goldfield concluded in his earlier work on urbanization in Virginia. Having demonstrated in two previous studies that Southern cities were an important element in a national urban network, he now asserts that the South has produced a distinct variant of the modern American metropolis. Goldfield's *Cotton Fields and Skyscrapers* is what he calls an "extended hypothesis" postulating that a rural life-style, racial arrangement, and colonial economy combine to create an environment where the "worlds of the cotton field and of the skyscraper are essentially the same" (p. 11). Abandoning urban growth models as a frame of reference, Goldfield focuses on the influences of region and culture in shaping three centuries of Southern city life. In so doing, he traces the development of cities chronologically through chapters describing the colonial

era, the antebellum period, the New South through 1920, and the modern South to 1980.

Goldfield initiates his account with the givens of climate, geography, and geology that made the South a fertile place for farming but not for urban growth. In the colonial period, the success of the planter-merchant obviated the need for market towns, while life in the larger coastal and river cities followed the rhythm of the agricultural season. Goldfield is convinced that the luxuriant growth and the garden plots of these cities distinguished their environment from that of the more congested landscape of their faster growing Northern counterparts. In the antebellum era, the success of cotton as a staple crop discouraged the development of diversified agriculture and monopolized capital that could have been invested in industrial and mercantile growth. The result was, in Goldfield's phrase, "urbanization without cities," the proliferation of small towns that had a uniquely agrarian look but not of larger cities and metropolises (p. 32). Urban growth after the Civil War continued to reflect the domination of cotton: towns, now slightly larger, were still agricultural marketplaces; those moving to cities brought with them the values of the countryside; and the poverty of rural farm holdings was reflected in the lack of attention to physical amenities in cities. Since 1920, cotton has lost its agricultural dominance, causing a vast migration from farm to city and elevating the South to the rank of an urban region. Yet, Goldfield notes that small cities remain the characteristic urban settlement, and even the large, sprawling metropolises of Atlanta and Houston are not "real cities" because they exude rural not cosmopolitan values.

An enduring biracialism, Goldfield's second regional characteristic, is rooted in the introduction of African slavery during the colonial era. By the early nineteenth century, one-third of the South's population was black as were 20 to 40 percent of its urban residents, creating a racial mixture that set the region and its cities apart from the rest of the nation. The postwar replacement of slavery with peonage and sharecropping in the countryside and the color line in cities provided enough flexibility for many blacks to migrate to urban areas. Yet the system of segregation not only kept most blacks in rural poverty by restricting economic advancement, but also slowed urban growth by depressing wage scales, hampering capital accumulation, and lowering consumer demand. In the twentieth century mechanization and federal farm policies displaced the black agricultural work force from the Southern countryside, making race a national not just a regional urban issue. It was Southern cities that produced the leadership for the assault on the color line during the last two decades that caused a dramatic reshaping of life in the South. Still, Goldfield be-

lieves that despite the advances of integration and the election of black mayors, Southern cities continue to be burdened by the enduring economic gap between blacks and whites.

The distinctiveness of Southern cities, in Goldfield's argument, is also a direct result of the subservient role of the region in the national economy. If the banks of London controlled the course of Southern economic development in the eighteenth century, those of New York did so in the nineteenth century. In spite of the continued and quite visible effort of urban boosters, manufacturing failed to develop in the region and staple crop agriculture dominated the economy. The Civil War was but an interlude in the process. While the late nineteenth century did bring textile plants to the Carolina piedmont and steel mills to Birmingham, these industries did little more than process the region's raw materials. Goldfield emphasizes the continuing pattern: the profits of ownership of factories and of railroads went North, leaving little for investment in other industries that would generate city growth. The growth of the "sunbelt" South of the twentieth century, facilitated in part by the federal government, has not really increased the economic independence of the region. New Deal agricultural policies have helped to diversify and mechanize agriculture, the wartime economy brought the benefits of Southern military installations and an end to freight rate discrimination, and civil rights laws have removed barriers to the expansion of the operations of national corporations in the South. But in Goldfield's scheme, the South remains a colony because the financing for urban and industrial development still comes from outside the region.

While there are a number of provocative generalizations that evoke debate in this study, Goldfield's argument for the distinctiveness of Southern cities serves a variety of purposes. First, it invites the testing of his "extended hypothesis" in other regions. Are there distinguishing cultural features that differentiate cities in the Northeast from those in the Midwest, West, and Northwest? Second, it provides the most readable account to date of Southern urban development that should be of use in both urban and Southern history courses. Finally, the bibliographical essay, provided in lieu of footnotes, serves as an excellent account of the literature and as a guide to the research potential in Southern urban studies.

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WARREN W. HASSLER, JR. *With Shield and Sword: American Military Affairs, Colonial Times to the Present.*

Ames: Iowa State University Press, 1982. Pp. x, 462. \$29.50.

Over the years some excellent general histories of the army, navy, marines, and air force have been published. There have also been some efforts at synthesizing military policy and strategic thought. Most of these general works are out-of-date and none has attempted to weave together the disparate strands of the history of the individual services or to integrate the story of armed conflict with the general history of the United States. The reasons for the paucity of general works are not far to seek. The three services have had different roles and missions, separate administrative structures, and distinct mores and traditions. In addition to trying to deal with the large body of literature pertaining to the individual services, the would-be synthesizer must ask what it is he will synthesize. Will it be institutional development, leadership, policy, operations, strategic thought, or what? In his attempt to face such questions Warren W. Hassler, Jr. maintains that "The time is overdue for a documented study that discusses the participation of significant individuals interacting with American military operations on land, sea, and in the air and their search throughout history for a viable military policy in times of peace and war" (p. x).

Although the task he sets for himself is formidable, Hassler attains a high degree of success. The whole compass of military history from colonial times to the present is covered and for the most part with good proportion and balance. The military buff expecting to hear the distant roll of the drum and the crack of rifle fire will not be disappointed. Especially good are chapters on the military reforms following the War of 1812 and the principal operations of the Civil War, a period in which Hassler is thoroughly at home. The thumbnail sketches of leaders, military and civilian, add human interest and zest to a narrative that sweeps the reader along.

Quite traditional in its approach, this book offers little that is new or daring by way of interpretation. Its general theme is that the United States over most of its history neglected its military establishment in times of peace and had to improvise in times of war—a thesis set forth by Emory Upton before World War I. In recent years some historians have sought to avoid Upton's oversimplifications and have tried to integrate military affairs with both diplomatic and social history. Hassler seems little affected by these tendencies. For the period since World War II he has a single chapter entitled, "Armed Forces in Global Commitment." During this period the advent of nuclear weapons, the emergence of a bipolar world, and the Cold War gave rise to a renaissance of strategic studies and a massive and impressive literature was produced

principally by the political scientists. Although it cannot be said that Hassler ignores this scholarship, it seems to this reviewer that he gives it inadequate attention, even for a rapidly paced survey.

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JOHN PUTNAM DEMOS. *Entertaining Satan: Witchcraft and the Culture of Early New England*. New York: Oxford University Press. 1982. Pp. xiv, 543. \$25.00.

With the publication of this book, the historical study of American witchcraft finally comes of age. Except for the 1692 outbreak at Salem, the subject has lagged behind European and non-Western witchcraft studies. *Entertaining Satan* integrates witchcraft into the broad examination of what its subtitle terms "the culture of early New England." The book displays the virtues we have come to associate with the work of John Demos: multidisciplinary skill combined with humane sensibility, venturesome theoretical flights with rich vignettes that vividly evoke patterns of everyday life.

Entertaining Satan is organized with careful symmetry. Each of its four sections contains two chapters of case studies and a final chapter of collective analysis. Part 1, "Biography," confirms that the typical "witch" was female (by a 4:1 ratio), aged forty to sixty, of relatively low social status, and with a history of contentious behavior. Part 2, "Psychology," analyzes the inner world of the accusers, who fall intriguingly into two main groups: "menopausal" women and men in their twenties. Here Demos turns to modern psychoanalytic theory—especially the work of Heinz Kohut, whose "self-psychology" school has emphasized the childhood struggle for "autonomy" rather than the oedipal conflicts stressed by Freud. Demos finds Kohut's theories especially resonant for Puritan society, which systematically suppressed "autonomous strivings" (but not genitality) from infancy, in the name of social harmony and consensus. Part 3, "Sociology," returns to more conventional terrain and examines interactions between accusers and accused in the structured setting of particular Puritan communities. Demos evokes that combination of rigid hierarchy and corporate interdependence, systematic repression and densely personalized relationships, which characterized these communities; and he shows how these structures were simultaneously violated and confirmed by "witchcraft." Finally, part 4, "History," investigates these communities over time. Here Demos proposes an inverse correlation between witchcraft and other forms of local conflict: witchcraft prosecutions flared up only *after* other crises had died down.

It is when he infers the childhood origins of

individual fantasies that Demos's speculations take him further than many readers may be willing to follow, because the evidence is often scanty and open to multiple interpretations, and also because these passages are uncharacteristically dominated by technical language. Furthermore, it could be argued that Kohut's transcultural claims about the urge for autonomy are as deeply rooted in the culture of the mid-twentieth century as Freud's oedipal theories were rooted in the culture of the late nineteenth. Nevertheless, Demos's connections are usually fascinating. Witchcraft accusations were typically made at a time in the accusers' lives when their wonted social status had been jeopardized: hence midlife women whose childbearing years had just ended (Demos hypothesizes that the physical production of children—as distinct from their ongoing nurture—gave colonial women their basic sense of worth) and young men who were ready to establish their own households yet who remained dependent on their parents—and ready to lash out at women reminiscent of the mothers who had once suppressed their own autonomy. Demos finds it significant that many accusations involved injuries to cattle or to very young children, both of which were associated with nurturing motherhood, and specifically with *milk*. (Here, characteristically, Demos pauses to take his readers on a "brief excursion into livestock history," probing estate inventories for evidence that only cows—but no other domestic animals—were given familiar names [p. 146].) Demos thus portrays a powerful set of inner and outer relationships in which accusers and accused refracted each others' most antisocial longings. It was a complex system in which witchcraft prosecutions helped to define (and thereby to renew) "the inner bonds of community"—"to unify 'us' by lopping of one of 'them'" (p. 305).

The presentation is supple and compelling. It is also significant that this self-sustaining system fits the anthropological theory of "functionalism"—the idea that witchcraft prosecutions serve to preserve the social order rather than to undermine it. As Demos acknowledges, the functionalist model works better with "small trials" than with "large panics." This is Demos's rationale for his otherwise surprising decision to leave out of the book any evidence generated by the 1692 trials at Salem—the biggest outbreak of all, but one that does not easily fit a functionalist model. But there is still another problem with that model: it assumes an identity of values between official Puritan social theory (the ideas of John Winthrop and Cotton Mather) and the folk culture of early New England—what the clergy and magistracy meant (and feared) by witchcraft was also what people meant at the neighborhood level. But there is evidence within *Entertaining Satan* that the Puritan legal system frequently acted to contain

or even challenge the popular witchcraft "lore" that lay at the heart of these "small trials." I am struck by the frequency with which (except in 1692) the courts were more lenient than the "people": by the number of directed acquittals, reversed verdicts, commuted sentences, or (most commonly) cases that never even came to trial. Demos pays little attention to the actual *disposition* of witchcraft cases, and thus precludes considering the possibility that the Puritan legal and theological system served to assert class functions as well as to reinforce shared values. It would have been worth asking what was specifically "Puritan" about New England witchcraft, and what was simply the product of old European peasant traditions. But that is asking John Demos for a different book, one he might not have cared to write. Nobody else could have produced *Entertaining Satan*. Its publication testifies to the ongoing growth of a uniquely talented historian.

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CHARLES E. HAMBRICK-STOWE. *The Practice of Piety: Puritan Devotional Disciplines in Seventeenth-Century New England*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg. 1982. Pp. xvi, 298. \$28.00.

Despite the various shifts that the reputation of New England culture has undergone in three centuries, the popular image of the Puritan as a man of strenuous and disciplined piety has remained dominant. Yet the torrent of Puritan studies appearing since the 1930s contains inadequate examination of the institutionalized mechanisms of New England spirituality or the quality of that private "heart religion" that they were intended to sustain. This deficiency is especially notable since in the same period Catholic, Anglican, and English Puritan devotional systems have received careful analysis. *The Practice of Piety* now attempts to repair the neglect and in so doing to reconstruct a more balanced portrait of the Puritan that allows proper emphasis on his sometime role as a Protestant contemplative.

Accordingly, Charles E. Hambrick-Stowe sees his work as a corrective to historians who have dwelt on Puritanism's intellectual achievements. On less fundamental details he also differs from more recent scholars (Louis Martz, Horton Davies) in stressing continuities with medieval piety and devotional techniques. The saints' emotional experience, he argues, and the elaborate scheme of exercises that were calculated to supply religious dynamic to their lives were central realities of Puritan culture. Dominant in that scheme was the reiteration of the

pilgrim motif, the ever-continuing striving after God that terminated only in death and the soul's ascent to glory. "Preparation," therefore, was not so much a stage in life leading to conversion or salvation as it was the Christian life itself; Thomas Shepard recommended getting Christ in *renewed* conversions. Preparation for morn, for the coming year, for the Sabbath, for the sacrament, for the final hour—Puritan spirituality had a pronounced eschatological character. All life was to be employed in vigilant anticipation of eternity.

Almost half of this volume is reserved for systematic examination of the components of this regime of spiritual living. While the discussion of "ordinances of public worship" is a useful survey, particularly respecting the emergent importance of covenant renewal in the late seventeenth century, the chapters on private devotion are probably the most interesting. Not all such devotion was secret. Family prayer, neighborhood gatherings, meetings of Harvard students, of "Gentlemen," of "devout Women," of "the Society of Negroes" emphasize the social but laic nature of much of this piety. "Closet exercises"—reading, meditation, and prayer—had surprisingly strong links to pre-Reformation practices, especially in the widespread use of short outbursts of ejaculatory prayer. The Puritan's high regard for diary keeping could yield a record of morbidity and guilt (as in Michael Wigglesworth) but more often of reassurance and joy as in Increase Mather's "soaring Interviews with Heaven."

Although a penultimate chapter provides a fitting conclusion to this analysis by narrating the "devotional crisis of the second generation" (associated with the Half-Way Covenant), earlier historical tensions in patterns of piety that contributed to "left-wing" Puritan dissent are only mentioned and the focus is on the approved spiritual disciplines of Massachusetts Bay. (This perspective may account for the odd reference to William Aspinwall as a "non-Puritan" [p. 191] and the failure to mention Roger Williams's celebrated guide for his wife, *Experiments of Spiritual Life and Health* [1652].) And despite emphasis on the laic character of Puritan piety, reliance is heavy, perhaps inevitably, on the usual clerical figures—Cotton, Eliot, Hooker, the Mathers, Shepard, Taylor, and the like. Nonetheless, Hambrick-Stowe has made an unusually wide canvass for evidence, drawing imaginatively on almanacs, election and militia sermons, and captivity narratives as well as more obviously devotional materials. The work presents a comprehensive and sensitive exposition of the interior life and devotional rituals that refined the Puritan saint and lent vitality to much of his culture.

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GLORIA L. MAIN. *Tobacco Colony: Life in Early Maryland, 1650–1720*. Princeton: Princeton University Press. 1982. Pp. xv, 326. \$30.00

Gloria Main has written a fine book, one that richly portrays everyday life in colonial Maryland in its great transition from a fluid frontier society in 1650 to an inegalitarian one in 1720. Drawing on the vast body of probate records, she has patiently and systematically reconstructed the social life, material culture (everything from houses down to the most mundane of household items), and consumption-investment strategies of Maryland's tobacco planters. Main has brought the records of the dead to life, and in so doing she offers new insights and some fresh question about colonial society.

The first part of this book is about the declension of Chesapeake society and its causes. Main attributes the emergence of an inegalitarian economic structure to the twin engines of a tobacco staple and, more modestly, demography. The plot will be familiar to those abreast of Chesapeake scholarship. By the mid-seventeenth century, the tobacco economy had sucked in everyone with its promises of profit and social mobility. Once committed, however, the Chesapeake planters found themselves entrapped by the severe depression of the 1680s and three further decades of stagnation in the European demand for tobacco. Hard times forced adjustments, particularly in the transformation of the labor force from white indentured servants to black slaves. Whatever the causes of this rapid change in labor force (Main sides rather uncritically with the view of rational economic choice and relative price changes), the adoption of slave labor facilitated the growth of inequality. Poorer planters, lacking capital or credit, were precluded from this labor market, while affluent planters purchased slaves and accumulated wealth in disproportion to their neighbors. Income distribution was further exacerbated, in Main's view, by the demographic change from an immigrant to a native-born population. The convergence of population growth and a stagnating tobacco economy made unlikely much upward mobility by poorer planters. Economic inequality was, by 1700, a fact of life in the upper Chesapeake.

The second part of this book details the everyday life of tobacco planters, and it is here that Main makes an original and most important contribution. Collating literary evidence with her systematic analysis of probate records, Main examines the ways of life of the poorer, middling, and affluent planters. Her discussions range over such matters as their houses, furnishings, economic activity, diet, and lifestyles. A short review cannot hint at the richness of Main's reconstruction, but the main point seems to be that despite the increasing economic inequality, Maryland's rich planters did not live very differ-

ently from their neighbors. Main is somewhat perplexed by the spartan-like lifestyles of Maryland's first elite. Somehow, in her judgment, they did not behave well; they lived beneath their station; these clearly were not the Chesapeake gentry of a later generation. Main finds their capitalist ascetic a bit perverse and a little sad—perverse in that the elite “had the wherewithal to live well by the standards of their time,” yet most “lived in a style as plain and simple as that of their less affluent neighbors” (p. 225–26), and sad in that their style of life represented a triumph of crassness over contemplation, utility over decoration, disorder over order, and the philistine over the aesthete. I would put it more baldly: Main has discovered that this first Chesapeake elite had no class.

Main's discovery is of considerable significance for the interpretation of Chesapeake society, and it is regrettable that she does not pursue its causes and implications. Why did this elite stop short of the class accoutrements that they had earned? Contrast them, for example, to Wilbur Cash's hardscrabble cotton planters in the nineteenth century who, in one generation, rose from rags to riches and from redneck to blueblood. The difference, it seems to me, has to do with contrasting economic curves—Cash's planters living on a long upward curve of prosperity while Main's lived on a curve hell-bent for collapse. Chesapeake planters who had profited from the adversity of a tobacco economy stood prepared to abandon it if necessary. It is the sense of impending disaster in the tobacco economy that Main misunderstands. Curtains and chamber pots counted for little when the basis of elite livelihood stood in jeopardy. Class behavior awaited the more halcyon days after 1720 when the hegemony of tobacco was assured. Main's story, unfortunately, ends just when it gets interesting. We may hope that she will soon return to her discovery of this classless elite and how they and their descendents acquired class, behaved “well,” and assumed command of Maryland's political economy.

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JAMES A. MULHOLLAND. *A History of Metals in Colonial America*. University: University of Alabama Press. 1981. Pp. xiv, 215. Cloth \$17.95, paper \$8.95.

A History of Metals in Colonial America accomplishes exactly what the title promises, a careful and thorough survey of the role of metals and metal production in the British colonies with few surprises for the reader. Although James A. Mulholland's claim that historians have largely neglected this topic is overdrawn, as his own bibliography and sources illus-

trate, he has succeeded in his purposes of providing "an additional dimension for the understanding and interpreting of the industrialization of the United States" (p. ix). Few will be surprised or disagree with his thesis that metals played a central role in the culture of Western civilization prior to the discovery of North America and were absolutely vital for the colonization and development of the English colonies in the New World.

Mulholland begins his survey at the beginning of the sixteenth century with the publication of Thomas More's *Utopia* and concludes with the discovery of gold in western Carolina in 1799. The search for and discovery of silver, lead, copper, and iron, and their exploitation and significance in the colonial economy are systematically discussed. Little or no silver or gold, however, was discovered during the colonial period. And since copper production never reached significant levels, Mulholland quite correctly confines most of his discussion to iron production and metal fabrication, both of which achieved levels of major importance in the period.

From rather humble beginnings in the mid-seventeenth century with the erection of iron furnaces at Braintree and Saugus in Massachusetts, iron manufacturing rapidly achieved major significance both in the domestic economy and in the British imperial scheme. By the end of the seventeenth century iron works in colonial America had assumed the basic "iron plantation" configuration that would characterize their operations well into the nineteenth century. The production of charcoal iron required sizable investments, complex technology, skilled labor, production for future markets, and sophisticated entrepreneurial talents. Yet by the end of the Revolution, more than eighty furnaces had been built with an average production of over three hundred tons a year. The census of 1810 listed more than 150 furnaces, nearly 500 forges and bloomeries, 34 slitting mills, and over 400 naileries. The United States, since 1790, had annually exported pig iron. Clearly in colonial terms iron works were, by necessity, big business. The scale of their operations meant that early iron furnaces produced more than local markets could consume. By the second decade of the eighteenth century "a steady stream of pots, pans, firebacks, and implements" domestically produced began to compete for the colonial market and the "combined impact on the colonial market drew the attention of the British government to the economic significance of iron in America" (p. 73).

In 1750 by means of the Iron Act the British attempted to incorporate the colonial iron industry in the imperial economy by encouraging the production of pig iron and restricting further erection of rolling or slitting mills. I am inclined to agree with Mulholland's conclusion that British acts of trade

and navigation had little impact on the development of the iron industry, certainly less than the shortage of capital, poor transportation, and limited markets.

Indeed, I have only two minor quarrels with what is otherwise a carefully researched and valuable survey. Mulholland makes too much of More's *Utopia*; I doubt if many Puritans came to the New World expecting to build a perfect society, and I really do not believe that the American Revolution was "a greater utopian venture—the creation of, as they conceived it, the perfect polity" (p. 118). Although the extent of the market is a decisive factor in economic development, it should not be carried to the extreme of Mulholland's plea for protectionism. These are minor flaws, however, in an otherwise well-done and useful survey of metals in colonial society.

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MICHAEL W. ZUCKERMAN, editor. *Friends and Neighbors: Group Life in America's First Plural Society*. Philadelphia: Temple University Press. 1982. Pp. v, 255. \$27.50.

In a provocative but valid introductory essay to this anthology, Michael W. Zuckerman complains that the middle Atlantic area of British America long has suffered comparative inattention from American historians. They have showered their favors instead on the South and New England. This, Zuckerman finds unjustifiable in the light of "the actual unfolding of American destiny" (p. 4). Concerning New England, Zuckerman insists that "no colonial region has been less relevant" (p. 4). Along with such distinguished observers of the American nation's development as Hector St. Jean de Crevecoeur, Frederick Jackson Turner, Woodrow Wilson, and Samuel Eliot Morison, Zuckerman claims that typically American patterns emerged first in the Middle Colonies of New York, New Jersey, and "especially" Pennsylvania. "The wonder," he writes, "is that we ever thought otherwise" (p. 11).

One of the several ways in which this area was a precursor of the nation was in its "cultural pluralism" that Zuckerman asserts "only came to characterize the rest of the country in the nineteenth century" (p. 5). It is around this theme that Zuckerman arranges eight essays by University of Pennsylvania graduate students.

Each essay focuses narrowly on a particular point. Barry Levy emphasizes the role of the Quakers in "The Birth of the Modern Family." Valerie G. Gladfelter discusses the "Rising Individualism in the Burlington, New Jersey, Friends Meeting." An explanation of the extent and ways in which Quakers participated in the New Garden, Pennsylvania,

Friends Meeting is the subject of Susan F. Forbes's "Quaker Tribalism." Nancy Tomes presents another aspect of the Quakers' tribalism as she discusses "... Visiting Patterns among Women" in the Philadelphia Friends Meeting. Broadening the anthology's ecclesiastical scope is Deborah Mathias Gough's "Roots of Episcopalian Authority Structures . . . in Colonial Philadelphia." Wayne Bodle exposes the diversity of the locality around the Continental Army's winter camp at Valley Forge in 1777-78. In Ned Landsman's essay the "Scottish Proprietors" receive overdue attention for their "Planning of East New Jersey." The interaction of ethnic and religious groups in the "Pennsylvania Town" of Reading is Laura Becker's topic.

These essays demonstrate that pluralism can be defined in different ways. The variety of political and social forces in the vicinity of Valley Forge became obvious when the rebel troops spent a winter there. The Friends were an important component of the area's religious pluralism but varied in some ways from meeting to meeting and even within individual meetings. It is only in the traditional understanding of the term that some readers might wish for greater breadth of coverage. Exactly half of the essays emphasize the Friends, while other religious groups prominent in the Delaware Valley in the eighteenth century, such as the Presbyterians and Baptists, are hardly mentioned. The same criticism could be applied to the volume's ethnic scope. The numerous German settlers receive attention in only one essay, and the significant Scots-Irish are noted only in passing.

Nevertheless, this anthology is a welcome addition to the growing body of historical literature on the Middle Colonies. Its editor and contributors deserve much credit for their interpretations of "America's First Plural Society."

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CHARLES W. AKERS. *The Divine Politician: Samuel Cooper and the American Revolution in Boston*. Boston: Northeastern University Press. 1982. Pp. xii, 445. \$22.95.

This is a biography of the pastor of the Brattle Street Church. The subtitle is appropriate because 83 percent of the text deals with Samuel Cooper's role in Boston from 1760 to 1783 when he died. Charles W. Akers's aim is to illuminate and elevate Cooper's importance in Boston's Revolutionary movement by analyzing his correspondence, ferreting out his behind-the-scenes politicking, and identifying the anonymous letters he authored.

Brattle Street Church had been established by

liberal merchants reacting against the pietistic faction of the Mathers in the 1690s. One of the founders was Thomas Cooper, the grandfather of Samuel and a well-to-do merchant. When pastor William Cooper, his son, died in 1743, young Samuel, his grandson, was chosen successor and colleague of aging Benjamin Colman who soon died. Colman and William Cooper had steered the church through the Great Awakening by going with rather than against the revival mood. They were classified as New Lights, although their style of preaching was subdued and sentimental when compared to the sons of thunder itinerating in New England.

Samuel was an able and popular successor to his father but could not be described as a New Light. In fact, in almost all respects he was an Old Light. He emphasized the simplicity of the Bible and avoided theological speculation or any controversy in the pulpit. Throughout his life he remained a moderate Calvinist who avoided doctrine and gave no hints of any drift from orthodoxy. In the post-Awakening generation he was able to maintain full fellowship with Calvinists as well as liberals. Like other Old Lights in port towns, he was sensitive to the competition of the Church of England. He differed from the average Old Light chiefly in his talent for preaching. From his pulpit came a bland Christianity beautifully delivered. "Silver-tongued" Sam (p. 23) rendered a very "sweet gospel" (p. 61) that echoed the feelings of Colman and his father and anticipated the urban ministers of the nineteenth century.

Akers emphasizes Cooper's pragmatism in the pulpit. He agreed with the Whig elite of Boston who were his parishoners that pursuit of private interest was conducive to the public good. Cooper was a precursor of the positive thinkers, an optimist who sought economic progress accompanied by stability. When divisions developed after the Confederation went into effect in 1781, he sided with the nationalists rather than the localists. These attitudes made him a good friend of Ben Franklin and the French alliance.

Akers has retold very well the familiar story of Revolutionary activities in Boston from 1760 to 1783, using the Cooper materials and integrating his subject with the events. Thus Cooper's role in the proceedings has been revealed and enhanced. Because Cooper was a pragmatic man of the cloth who preferred to work behind the scenes, however, he must remain in a secondary role compared to John Hancock, James Otis, and the Adamses. It seems only fair that men who stuck their necks out by taking public stands at the time should receive more credit.

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MILTON E. FLOWER. *John Dickinson: Conservative Revolutionary*. Charlottesville: University Press of Virginia, for the Friends of the John Dickinson Mansion. 1983. Pp. xii. 338. \$27.50.

The appearance of Milton E. Flower's *John Dickinson* is a special event because no other full-length biography of Dickinson of Delaware and Pennsylvania has been published since Charles J. Stillé's. In the years immediately after 1891, however, a relatively large number of shorter evaluations of Dickinson's career appeared, which reflected the then-current interest in his patriotic accomplishments. In recent decades his prominence as a leading conservative in the legislative debates of the Revolution and Confederation has inspired investigation of those important ideas. Many of his personal motives and actions, nonetheless, remain obscure due to the scarcity of available manuscripts. More documents are also necessary to explain his idiosyncrasies. This new biography addresses itself to some of these points, but it is basically an assimilation of studies since 1891 without reaching the cutting line of current research. Its assets are many, including an attractive format. The book is a handy reference, which will undoubtedly be used by the Friends of the John Dickinson Mansion, the sponsors of the biography, in disseminating information on their patriot's career.

Because of the fast pace of the biography, many interesting problems raised by Dickinson's career are only lightly handled by Flower. In the pre-Revolutionary period, for example, how did Dickinson's relations with Joseph Galloway, Benjamin Chew, and Benjamin Franklin influence the colony's politics? What strengths and weaknesses of character and position did he exhibit? Dickinson had a solid social and political base as the son of a wealthy Delaware planter, an English-trained lawyer, a Quaker in spirit if not in practice, and a member of Philadelphia's elite through an excellent marriage. He was one of the leaders of the proprietary faction of the colony—a spokesman, writer, and parliamentarian for the group. Still, he was able to hold power only briefly against the combined forces of Franklin and Galloway, who favored royal over proprietary rule of the colony. Although Franklin and Galloway did not win their campaign for a change in government, Dickinson was kept by them (or himself) from office. Why? Early in the struggle with Great Britain he became aroused over imperial policies and saw a unique chance to attack his Pennsylvania rivals. His *Letters from a Farmer* in 1768 not only accomplished that purpose but also gained him great prestige elsewhere in the colonies. But the fortunes of the imperial controversy associated him with the radicals and frightened him. His relations with Galloway remained hostile, but the

reasons, other than personal rivalry, are not readily understandable. Both men had much in common in their social positions, professions, basic ideas about good government, and views of life. What in their personal make-up permitted Dickinson to swim reluctantly with the current and become a revolutionary and convinced Galloway to resist firmly the current and deepen his loyalist beliefs? Was it their education, their friends and wives, or just the accidents of fate? Answers to these and other questions would illuminate the Revolutionary movement in Pennsylvania and provide a better understanding of Dickinson's personality and presence, which contributed to his later election as president of Delaware and Pennsylvania. These comments lead me to observe that the major weakness of the biography is its narrative format, which does not provide the necessary opportunity to explain why certain events happened. The light pen, nonetheless, sketches fascinating pictures of Revolutionary life in Delaware and Pennsylvania and encourages one to appreciate John Dickinson as a conservative patriot.

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ROBERT L. HAMPEL. *Temperance and Prohibition in Massachusetts, 1813–1852*. (Studies in American History and Culture, number 32.) Ann Arbor: UMI Research Press. 1982. Pp. xvi, 237. \$39.95.

Robert L. Hampel joins a growing number of historians who seek to rescue the temperance movement from the condescensions of the post-prohibition era, although he believes that recent revisionist studies have relied too heavily on the rhetoric of reformers. In contrast, Hampel's work is quantitative in emphasis, and its strengths (and weaknesses) flow in some measure from this methodological choice. Manuscript sources and pamphlets are not neglected, but eighty-six tables conveying data, mainly on roll calls, temperance membership, and electoral politics, provide the backbone of the study. Hampel ransacked the archives of Massachusetts to find an unprecedented quantity of data. Specialists in the field are indebted to him for the most detailed available case study of a state temperance experience, and for unearthing so many fine sources.

Hampel's conclusions are often presented negatively. The reformers of the early Massachusetts Society for the Suppression of Intemperance were not anxious conservatives. The formation of the American Temperance Society (1826) did not mark a sharp break with earlier practice. The doctrinal conflicts of the 1830s were less divisive than previously thought. The "social control" model offered by (unspecified) historians does not fit the data. Rather, temperance was a heterogeneous reform, a

movement of considerable complexity centered around the "influence" of "respectability."

Other conclusions confirm recent scholarship. Hampel correctly criticizes symbolic and status revolution analyses and demonstrates that temperance was not tightly linked to Whiggery or evangelicalism. Some findings (for example, his social analysis of the Washingtonians) are broadly accurate, but not so novel as Hampel sometimes suggests. Enforcement has not been "entirely neglected" by earlier scholars, nor has "no work . . . ever examined the enemies of temperance" (p. 5).

Hampel's points are presented in a matter-of-fact form and fired off with the rapidity of machine-gun fire. He concentrates on "who" and "how" questions, believing that too much attention has been devoted to motivation (p. 5). Yet Hampel abdicates many crucial questions of causation and elsewhere reduces "why" questions to "how" questions. Thus the shift to prohibition after 1845 is explained in terms of the internal tactics and political position of temperance reform. Changes in external social conditions (an increase in crime, for example) are mentioned but never pursued (p. 48 and following).

Hampel's statistical methods are more sophisticated than previous works in this field, but problems remain because of the static and imprecise nature of the materials used. His finding that temperance did not correlate strongly with "manufacturing" areas (p. 93) is of inconclusive significance so long as his demographic and economic data do not measure the direction of historical change or the interpenetration of urban and rural society in a capitalist market economy. His interesting social profiles of drinkers are disappointing because that information is not set in a convincing social context. Finally, there is little treatment of women. Since Hampel states that 60 percent of members were women in the societies of the 1830s that he studies (p. 28), this is an unfortunate omission.

Despite these limitations, historians will be able to build on many valuable findings in this book. A similarly diligent study of temperance in other regions where its impact was weaker, for example, the South, would be welcome.

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MALCOLM D. MCLEAN. *Papers Concerning Robertson's Colony in Texas*. Volume 9, *October, 1834, through March 20, 1835: Sarahville de Viesca*. Arlington: University of Texas at Arlington Press. 1982. Pp. 610. \$25.00.

Malcolm D. McLean, then professor of Spanish and associate dean at Texas Christian University, began

a major publishing program with his compilation and edition of the *Papers Concerning Robertson's Colony in Texas* in 1974. Volume 1 appeared in that year, and since then the volumes have appeared yearly. The present volume, the ninth in the series, brings the saga of this early Texas colony through March 20, 1835. The colony originally occupied a region approximately one hundred miles wide and two hundred miles long situated in the Brazos River section of central Texas. Originally known as Leftwich's Grant, after Robert Leftwich, it developed at a time when Mexico looked favorably on Anglo colonization of its northern provinces. Later it became known as Robertson's Colony, after Sterling C. Robertson, who did more than anyone else to bring in an Anglo population to secure the claim on the grant.

The Robertson Colony was soon embroiled in a controversy, and ill will lingered among descendants of the original protagonists for decades. Indeed, one can find this ill will plainly evidenced in the publication of these papers. In brief, the controversy involved what Robertson and his descendants believed was nothing less than a criminal attempt by Stephen Fuller Austin, also known as the Father of Texas and one of the state's principal heroes, to appropriate Robertson's grant through misrepresentation to the Mexican government. All *empresarios* had to populate their land grants within a specified time. Robertson believed that Austin would carry word to the authorities that he had done so, only to learn that, in his opinion, Austin had reported exactly the opposite—to have the land awarded to himself and a partner named Samuel May Williams. Then, when Robertson traveled to Mexico himself to reclaim his land, and he did so successfully, he believed that an assassination attempt was inspired by Austin. This set the stage for a little publicized but bitter feud.

The Robertson clan has wanted to set the record straight for one hundred and fifty years, and this is a major motivation behind McLean's project. He makes no excuses for this; he wants his ancestor's role in the colonization of Texas recognized. Beyond that, his project does have considerable historical merit. Prior to his publication of the Robertson papers, the only Anglo *empresarial* papers in print were those of Austin, which were incomplete. McLean intends to publish every record available, and he and his wife have searched widely in this country and in Mexico and Spain for their sources. They have done a most complete and admirable job. In addition to editing the publication, McLean has translated all the Spanish-language documents. He is well qualified to do so, having headed the translators at the University of Texas Archives and having served as field editor in charge of Spanish translators for the Texas Historical Records Survey.

This is a life's work for McLean, and it is a labor of love. Several years ago he moved to the University of Texas at Arlington, and the press at that institution assumed the publication of the Robertson project. The volumes cover every aspect of the colony, beginning with Robert Leftwich's career in Virginia and other Eastern states, and the passage of the land grants through several owners to Robertson. The affair with Austin and Williams, Indian relations, government relations, and, finally in this ninth volume, the actual awarding of land grants totalling 474,630 acres to 132 individuals receive ample treatment. The index references 3,144 proper names associated with the colony, making it a gold mine for genealogists and historians alike. This volume is also much better illustrated than the earlier volumes, especially with maps, and McLean was able to correct maps for the General Land Office through his research in Mexican archives.

The project is, of course, still incomplete. Historians of the West, and especially historians of Texas, must applaud the availability of these papers for their value to all Texas topics for the period covered.

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EDWARD H. MADDEN and JAMES E. HAMILTON. *Freedom and Grace: The Life of Asa Mahan*. (Studies in Evangelicalism, number 3.) Metuchen, N.J.: Scarecrow Press. 1982. Pp. xii, 273. \$17.50.

Edward H. Madden and James E. Hamilton have written an estimable book about a significant figure in American religious history. Asa Mahan's life spanned the nineteenth century (1799–1899), touching many salient events of that era. As the first president of Oberlin College (1835–50) and of Adrian College (1860–71), he is remembered primarily for his advocacy of the rights of women to co-education on equal terms with men, of equal educational opportunity for blacks, and for the "new education." Asa Mahan is legitimately linked with Charles Grandison Finney, a colleague at Oberlin, who joined him in attacks on slavery. He is also known for his contribution to American philosophy as an articulate advocate of Scottish realistic philosophy. Among students of American religious history, Mahan is notable as a Protestant evangelical who joined the doctrine of Christian perfection with a commitment to radical social reform. Because of the many bases he touched, this study, according to the authors, "helps illuminate numerous strands of American philosophical, religious, intellectual, social, and political history" (pp. xi, xii).

The book proceeds chronologically as a narrative

history of the life and thought of Asa Mahan. As such, it makes a contribution to both American intellectual and social history. The book is constructed according to the geographical and social locations of Mahan. The seven chapters include: "New York Years," "Cincinnati," "Oberlin," "Leaving Oberlin," "Cleveland," "Michigan Years," "London and Eastbourne."

The first two chapters treat Mahan's evolution from his "Old School" Calvinist upbringing toward a "New School" emphasis on Free Will Trinitarianism and Christian perfection. The highlight was Mahan's role in leading the rebel students of Lane Seminary to Oberlin.

The most important section covers the Oberlin years where Mahan is seen as a quintessential representative of Jacksonian period revivalism when vital piety became the source of a radical social ethic. The Oberlin revival produced in Mahan a passion for social reform that engendered attacks on slavery, discrimination against women, intemperance, commercial corruption, militarism, and new religions.

As a philosophical deontologist and Christian holiness advocate, Mahan spoke ardently for peace and justice as a delegate to numerous national and international peace, women's rights, and antislavery conventions. During this period Mahan became a nationally known moral philosopher through the publication of *The Science of Moral Philosophy*.

Highlighting the personal and religious aspects of his life, the last section is the least satisfactory as a contribution to historical studies. By devoting only nine pages to the Civil War and Reconstruction with no mention of the social upheavals of the Gilded Age, we are left wondering if Mahan retreated from a vision of social reform to a privatized faith after the war. This account would suggest just that, and yet an interpretation of this shift is lacking.

Based on comprehensive primary source research and clearly written, this book is both substantive and interesting. Its primary weakness is the lack of a wider conceptual framework. Had the subject matter been related to the changing historiography of the nineteenth century, the book could have better illuminated both Mahan and the areas of his concern.

DONALD G. JONES

Drew University

ERNEST MCPHERSON LANDER, JR. *The Calhoun Family and Thomas Green Clemson: The Decline of a Southern Patriarchy*. Columbia: University of South Carolina Press. 1983. Pp. xv, 275.

This is an interesting book because it describes the family setting of a great American, John C. Cal-

houn. The focus is on Thomas Green Clemson who married Calhoun's favorite child Anna and whose financial resources shored up this impecunious Southern family. This Southern patriarchy is described, not analyzed, for the insights of a Drew Faust, as in her recent biography of James Hammond, are lacking here. This book rests on a thorough mastery of the private correspondence, and the reader must find his delight in getting to know very well the uncles and aunts, the children, and grandchildren, although Calhoun himself got more worry than support from the Calhoun-Colhoun clan.

Calhoun's wife was a Colhoun. Floride Bonneau Colhoun's brothers, John Ewing and James Edward, appear somewhat more interesting and certainly more financially secure than their nephews and nieces. Calhoun's eldest son, Andrew Pickens Calhoun, never made it as an Alabama cotton planter; Patrick never really succeeded as an army officer; John Caldwell did not produce citrus crops with any regularity in Florida; James Edward did not find gold in California; while Willie, the youngest son, died at twenty-nine on the eve of his career. Anna and Cornelia were the daughters. Cornelia was an invalid. Anna, the brightest and ablest of the offspring, occupies the center of this family history, and, because of her and her husband, the family home, Fort Hill near Pendleton, South Carolina, was preserved and given to the state. Clemson University flourishes as the chief legacy of this line of the family. One would therefore like to have known more about the founding and earliest years of that institution. The conclusion cuts short what many would have found an interesting denouement. There is also nothing about the grandsons who revived the family fortunes in the third generation. The theme of planters transformed into railroad men would have brightened the penumbra of this patriarchal world.

The author, who has produced admirable work on the antebellum South Carolina textile industries, on South Carolina and the Mexican War, on the last hundred years of the state's history, as well as editing a number of volumes including the diary of Floride Clemson, has throughout his career given us the world of Clemson (Pendleton) where he has taught many years. The best local history rises out of such attachments. There is no filiopietism here: just the bald and often pathetic story of children trying to come out from under the shadow of a masterful father. Clemson undoubtedly got his post as chargé in Brussels through his father-in-law's influence (nepotism was more acceptable in those days), but he was able, through possession of modest wealth derived from Pennsylvania sources, to maintain his distance, thus his individuality, his interests, and finally his important legacy separate from the Cal-

houn family. After all, the university is Clemson and not Calhoun.

GEORGE C. ROGERS, JR.
University of South Carolina

LEROY GRAHAM. *Baltimore: The Nineteenth Century Black Capital*. Washington: University Press of America. 1982. Pp. x, 335. Cloth \$22.50, paper \$12.25.

ROBERT J. COTTRILL. *The Afro-Yankees: Providence's Black Community in the Antebellum Era*. (Contributions in Afro-American and African Studies, number 68.) Westport, Conn.: Greenwood Press. 1982. Pp. xviii, 200. \$27.50.

Despite the apparent similarity of topics, these are two very different books; so different that they must be examined separately. The thrust of Leroy Graham's argument is that, throughout the nineteenth century, Baltimore was the "capital" of black (or, in the antebellum era, free black) America and that there was in that city a continuity of leadership in the fight to improve the condition of blacks. The human links in this chain were Elisha Tyson, William Watkins, George Alexander Hackett, and Isaac Myers. Tyson, of course, was white, but Graham sees his role as antislavery spokesman and financial supporter of black institutional development as initiatory in nature.

Although Graham tells us a good deal about these men, he does not attempt to tell us very much about the Baltimore black community. He has produced a sprawling manuscript that is heavily overburdened with names and minutia, and he gives an inordinate amount of space to the content of individual letters and speeches. There is frequent reference to (but little examination of) the Bethel AME Church but little more than mention of a dozen other antebellum congregations and many more in the last third of the century. Despite the extensive (and discursive) discussion of the 1879 struggle to desegregate the public schools and some comments on Watkins's activities, Graham makes no significant contribution to our understanding of black education, and such crucial matters as occupational opportunity, migration and persistence, and residential patterns and housing remain untouched. His analysis of the black in post-Civil War Baltimore politics is unnecessarily superficial, and includes no substantial examination of the nature and extent of, or reasons for, the splits among black Republicans or of the extent or significance of black voting. The black family is not mentioned and other black institutions (except for some tedious descriptions of the contents of some of the papers presented before black literary societies) remain unexplored.

These omissions might be justified on the ground

that the position of Baltimore as the "black capital" required Graham to take an external rather than an internal view of Baltimore blacks and their institutions. But he presents no evidence of a comparative nature to suggest that blacks in the rest of the country held this view of Baltimore and, indeed, it is much easier to make a case that in the antebellum era black eyes were more frequently turned toward Philadelphia and Boston than toward Baltimore.

Graham does provide a fair amount of biographical data on a number of Baltimore blacks, and some useful material on the school integration struggle and on black opposition to colonization. Regrettably, his other contributions are negligible.

Baltimore: The Nineteenth Century Black Capital has been presented by its publisher in an unattractive format and the copy furnished for review was so poorly bound that it began to shed pages at the first reading. There is no evidence of any copyediting and, consequently, the text is peppered with typographical errors and contains many dozens of sentences so awkward (and sometimes ungrammatical) as to bring the reader to a wrenching and horrified halt. It does not contribute to our understanding of Baltimore, its black community, or to the public's appreciation for historical writing.

One turns with relief to Robert J. Cottrol's *The Afro-Yankees*, not because it is flawless, but because it is a thoroughly professional, competent, solid, and useful contribution to the study of urban black communities. Cottrol's central thesis, addressed primarily in the first and last chapters, has to do with the forging of the black community. He sees this process as the result of the intertwining influence of racism and aspiration. Specifically, he argues that the relatively rigid class structure of pre-Revolutionary America—which viewed all servants, regardless of race, as members of a "deviant underclass"—inhibited the development of racial distinctions, at least in New England. The subsequent democratization of American society eroded these class distinctions, but opened the door to racial discrimination that, by lumping all blacks together, created an embryonic subcommunity. As black leaders developed institutions and enunciated ideals designed to elevate their standing in the whole community, they emphasized the social mores of the white middle class. Denied any realistic opportunity for economic or occupational advancement, they perceived this as the only alternative available.

The bulk of Cottrol's data is found in three substantial chapters (chaps. 2–4) descriptively entitled "Institution Building," "Political Activity," and "Occupation, Status, and Population." The first (and shortest) of these deals somewhat superficially with black families, churches, schools, and associations, and with Providence's two antiblack riots. The second is a careful description and analysis of the role

of Providence blacks in the Dorr rebellion, their subsequent political activities, and the unsuccessful struggle to integrate the public schools. The third is a quantified examination of property ownership, occupational patterns, and some demographic matters.

Here we encounter a few difficulties, often flowing from Cottrol's unfamiliarity with some broader contexts. It was not only the free black populations of Providence and other Northern cities, for instance, that were female dominant (p. 116); so was the national free black population. Consequently, some rethinking of Cottrol's suggestions about causation is necessary. Moreover, the dramatic increase in the number of black women in the work force, as reported by the census marshals, between 1850 and 1860 should not be attributed to the effects of the depression of 1857 (pp. 134–37). It was, rather, a statistical fiction created by the circumstance that the census enumerators were not instructed to collect women's occupations in 1850 and did so only by accident. Additionally, the deterioration of black male occupational opportunity between 1850 and 1860 (pp. 134–36) requires some examination, especially in light of the fact that figures published elsewhere show a slight but steady improvement during the years 1830–50. And finally, in too many instances, especially in the first chapter, any example from New England is assumed to be reflective of the Providence experience.

The Afro-Yankees is, on balance, a useful book. Some things are missing—it is regrettable that some attention was not given to black residential patterns and black mortality. It would be better if a few other things were missing—for example, the persistent use of the term "ward healers" for "ward heelers" and of the nonword "disenfranchisement." But Cottrol has certainly added to our understanding of Providence's antebellum black community and, thus, of other antebellum urban black experiences. And he is probably right in assuming that the best context in which to examine the development of racism and black community building is that furthest removed from the intrusion of the institution of slavery.

In the study of black urban history, as in much else in this life, you win some and you lose some. In this case the score was 1–1.

LEONARD P. CURRY
University of Louisville

WARREN F. SPENCER. *The Confederate Navy in Europe*. University: University of Alabama Press. 1983. Pp. xii, 268. \$19.95.

Except for several United States revenue cutters and a lighthouse tender seized in Southern ports, the

Confederate States at the beginning of the Civil War had no navy and no industrial base to build one. The Confederacy did have a navy department and, in Stephen Mallory, a capable secretary of the navy. It was clear to Mallory that if a force afloat was to be forged capable of seriously challenging the Union navy and keeping vital munitions and consumer goods flowing into the South, the ships would have to come from the yards and shipbuilders of England and France, the major European maritime powers. To this end James D. Bulloch and James H. North were dispatched abroad to procure, by purchase or building, ironclad warships and cruisers designed for commerce raiding. Bulloch and North were the first of a sizeable number of naval officers, including the world-renowned oceanic scientist, Commander Matthew Fontaine Maury, ordered to ship procurement duty in Europe. It is to this effort by the Confederate navy, its successes and failures, that Warren F. Spencer directs his attention.

With his focus on Confederate naval personnel in Europe, Spencer says he has written an account of sailors, ships, and diplomats. True indeed, and to this trilogy of coverage areas he could have added international law as it affected the interpretation of a neutral's obligations toward the belligerents. Queen Victoria's neutrality proclamation came within a month after the firing on Fort Sumter, and Napoleon III's government quickly followed. Spencer argues that the United States government strongly opposed the British neutrality proclamation because it bestowed belligerent status on the Confederacy. No note is taken, however, that when the Lincoln administration declared a blockade of the Southern coast, rather than announcing the Southern ports closed because of internal insurrection, belligerency had been tacitly conferred on the Confederate states. This was considerably before the British neutrality action.

The South's grand design, and Secretary Mallory's dream, in Europe was to obtain an ironclad fleet capable of crossing the ocean, raising the Northern blockade, and laying Boston, New York, and other Yankee cities under siege. This was never realized. Yet, despite numerous roadblocks including non-recognition by European governments, lack of money, no workable command structure, bickering over rank, and the failure of cotton diplomacy, the naval agents did manage to procure ships that sailed under the Confederate navy flag.

Spencer demonstrates clearly that the most energetic and effective naval agent in Europe was Commander Bulloch. He was responsible for the famous Confederate cruiser, the *Alabama*. It, and other commerce raiders obtained in England, played havoc with Northern shipping. Even more telling than the immediate loss of ships and cargoes to Southern cruisers was the flight of merchant ships from the

U.S. flag to the safety of foreign flag registry. This was a blow from which the American merchant service did not recover for decades.

This fine addition to Civil War naval and diplomatic literature, on a subject not heretofore synthesized, is deeply researched and full of facts. Spencer, however, does not seem to be completely at home with the peculiarities of what Samuel Eliot Morison called "Naval English." And, the writing style is somewhat unusual inasmuch as the narrative is riddled with author-generated questions. For example, on page 185 this series is found—"Why did Semmes fight? Why did *Alabama* lose? What happened to the men involved in the battle? What was the significance of the battle?" Sometimes the query is a simple, "Why had he done it?" To respond, any devoted detective mystery fan would ask the butler.

WILLIAM JAMES MORGAN
Naval Historical Foundation

RALPH MANN. *After the Gold Rush: Society in Grass Valley and Nevada City, California, 1849-1870*. Stanford: Stanford University Press. 1982. Pp. xv, 302. \$25.00.

This book examines the rise of two major gold mining towns in northern California, Nevada City and Grass Valley. The towns shared the luck that the gold resources on which both thrived lasted until they gained separate identities. That development took from 1849 to 1870, by which time Nevada City had emerged as a combination of placer mining town, commercial center, and county seat run by a native middle class, and Grass Valley flourished as an industrial town dominated by Cornish miners working its quartz ore. Dividing this span of twenty-one years into three equal parts, Ralph Mann describes in chronological sequence the changing composition of society and presents the emergence of permanent settlements, the search for prosperity and stability, and finally the appearance of two different towns with distinct social structures shaped by the type of mining best suited to each setting. A forty-four page appendix buttresses the prose, explaining the demographic, social, and economic changes with numbers arranged in forty-six tables that document the diverse turns these neighboring mining camps took on their way to becoming full-fledged towns.

In his approach the author follows the methods of the "new urban history." His major source is the manuscript census of Nevada City and Grass Valley for 1850, 1860, and 1870. From these enumerations he has extracted data for a computer program and through crosstabulation obtained information on the residents' sex, age, ethnic origin, family size,

occupation, and wealth on which to base his analysis of their activities and attitudes. These findings he weaves into a narrative that gets a touch of local color from newspaper reports and accounts of mines and towns. He focuses on issues of continuity and innovation, of custom and contingency. This mode of exposition produces a tightly written, skillfully argued monograph, which enables the author to assess quite effectively physical and social mobility, ethnic and spatial distribution, racism and work, and the transmission of culture. The author also frequently probes the questions raised in studies of mill towns and frontier markets. This allows him to relate his conclusions to models of social interaction discussed by other scholars and to stress the continuity of customs modified by the local economy, which accounts for the resemblance of Nevada City and Grass Valley to other small towns in the country.

This kind of parallel investigation somewhat broadens the perspective of a quite narrow study and gives it added significance as an attempt to relate an aspect of the California experience systematically to the larger field of United States history. Furthermore, it lightens the rather one-sided treatment of the complexities of urban life that bypasses a variety of sources of insight and information. With social-science models as the only frame of reference, the historical context of the northern mines is slighted. Diverse evidence is ignored, such as Lola Montez's activities, the "Plan of the Village of Grass Valley," or Heinrich Schliemann's caustic comment on Nevada City ("a small and extremely nasty place in the midst of a pine-forest"). The physical setting or architecture as a mirror of society receives little attention, and an inadequate map and three poorly reproduced illustrations do not help much. Only the figures of the population counts really seem to matter. The people themselves remain quite mute, even after the analysis of events when their passions ran high. The book makes an interesting contribution to social-science history, but the people remain distant.

GUNTHER BARTH
University of California,
Berkeley

H. CRAIG MINER. *Wichita: The Early Years, 1865-80*. Lincoln: University of Nebraska Press. 1982. Pp. xiv, 201. Cloth \$17.50, paper \$7.50.

The historiographical exhortation of recent years to urban and Western historians—study the city in the American West—has produced several important works on themes of Western urbanization. But there are still only a limited number of scholarly studies of individual cities, the traditional "urban

biographies," that for the East and the Midwest supplied the building blocks for the first generation of important historical works on American cities. This history of the first fifteen years of a frontier settlement that became Kansas's largest city—the first of two projected volumes—supplies a thorough, well-written history of the founding and early growth of an important metropolis of the plains. H. Craig Miner demonstrates continual awareness of larger themes of Western urban history, but his attention to these is not obtrusive. The work, written principally from extensive primary sources, stands as a lively narrative and social history of a place that in reality was almost as colorful as the legendary cow town of frontier marshals and mythical gunfights celebrated in story and song.

In an analytical concluding chapter, the author straightforwardly relates Wichita's early history to interpretative themes of Western urban historiography. As was the case on earlier frontiers examined by Richard C. Wade, Wichita was a "spearhead" in the growth of Kansas, not a product of agricultural settlement. Miner would agree with Lawrence H. Larsen that Western urban leaders consciously followed a policy of trying to be as "urban" or as "urbane" as their Eastern counterparts. Despite their emphasis on establishing cultural institutions, Wichita, like Kansas City, Denver, Cheyenne, or Seattle, was essentially an experiment in promotion and economic enterprise. Miner, as have other historians, accepts much of the boosters' assertion that superior promotion and community organization, not natural geographical advantages, determined the outcome of Western urban rivalries. He departs from the arguments of Robert R. Dykstra and accepts the earlier viewpoint of A. Theodore Brown and the present reviewer that Western towns, at least in their early years, were unified on promotional and economic issues related to their success and not divided by class interests within the community.

Throughout his study, Miner develops a neglected subject important to all cities but particularly so to those of the plains, Rocky Mountain regions, and the Far West: the role of federal policy in urban development. The removal of Indians from reserves in Kansas made Wichita possible. Capital, necessary for the Wichita town-building effort, came from trade with Indians cut off from their hunting grounds. Aid to railroads, liberalization of land sales, and scores of other federal programs contributed to the fact that Wichita by 1880 had begun to eclipse the then larger places of Atchison, Leavenworth, Topeka, and Lawrence, whose earlier histories had also been shaped by federal decisions. Miner develops the intriguing suggestion that the early years of Western towns like Wichita might well be "seen as exercises in colonial administration

centered in Washington and designed to tie private interest to public policy" (p. 172).

CHARLES N. GLAAB
University of Toledo

TED C. HINCKLEY. *Alaskan John G. Brady: Missionary, Businessman, Judge, and Governor, 1878–1918*. Columbus: Ohio State University Press, for Miami University, Oxford, Ohio. 1982. xvii, 398. \$40.00.

For a land as vast and with as varied a past as Alaska, there are few essential books on its history. Ted C. Hinckley's latest offering may not be as central to our understanding of the forty-ninth state as his *Americanization of Alaska*, yet it is a well-researched study that uses the multifarious career of John Brady as a vehicle to focus in greater detail on a range of Alaskan topics.

Presbyterian minister Brady arrived in Sitka in March 1878. Almost immediately he began a life-long love affair with the "Great Land" and developed a deep concern for its native peoples. Like most missionaries and the pre-eminent Sheldon Jackson, he subscribed to the philosophy of educating Alaska's natives to successfully adopt Western culture. Rebutting those who would decry this as cultural imperialism, Hinckley correctly contends that there was no better alternative.

After less than a year preaching the gospel, Brady forsook the ministry to enter business, first as a merchant and later as a lumberman. In 1884 he received an appointment as the first U.S. commissioner at Sitka, and in 1897 President McKinley named him governor. For nearly a decade, including the height of the district's feverish gold rush, Governor Brady was intimately connected with every aspect of Alaska's political and economic life. In relating Brady's career, Hinckley discusses Alaska's small but growing pre-Klondike economy and the boom that began almost simultaneously with his becoming governor; the frustrations of businessmen and settlers where federal policy made securing land title virtually impossible; the clash of white and native cultures, particularly the troubling effect of alcohol on native life; the paucity of social and economic institutions and facilities in the "Last Frontier"; and the ludicrousness of expecting a handful of officials to enforce laws, including federally-mandated prohibition, in a land so large.

Hinckley accurately portrays Brady as a virtuous Horatio Alger figure whose pluck and luck, rather than uncommon ability or intellect, determined his destiny. Brady displayed considerable energy as a businessman and promoter of Alaska's welfare, but he also benefited from having been adopted by a kindly Ohio judge, from gaining powerful contacts as a student at Yale, and from having the unstinting

support of Jackson, Alaska's influential general agent for education in Washington. Although a conscious do-gooder, Brady too-sanguinely identified his will with goodness and could be blind to how his actions harmed others. He overrode objections to locating his home on Indian cemetery grounds, squandered his family's savings, and encouraged friends to invest in a ruinous speculation.

Hinckley's writing is casual, sometimes graceful, and studded with colorful metaphors. He describes the boom town of Skagway as "the most parasitical of mushrooming spores fertilized by Yukon riches" (p. 160). The book would have profited considerably from tighter organization and the elimination of extraneous matter. More careful editing also would have deleted the archaic phrase—"the governor and his lady" (pp. 239, 289). Nevertheless, Hinckley's extensive research and sound judgment, along with the broad context within which he places Brady's career, makes this a valuable contribution to Alaskan history.

JAMES H. DUCKER
Anchorage, Alaska

LESTER D. STEPHENS. *Joseph LeConte: Gentle Prophet of Evolution*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press, 1982. Pp. xix, 340.

Joseph LeConte was not a giant in the annals of nineteenth-century science. He was, however, a solid contributor to the fields of geology and physiology, a popularizer of evolution, an indefatigable and beloved teacher for more than thirty years at the University of California at Berkeley, and a gentle and humane man. In all these respects, he is well served by Lester D. Stephens's sympathetic biography.

Born into a Southern slaveholding family, LeConte took a medical degree at the College of Physicians and Surgeons in New York in 1845, but science, not medicine, was his real love. After more than a year of study under the famed Harvard scientist Louis Agassiz, LeConte returned south to teach, first briefly at Oglethorpe University, then at the University of Georgia, his alma mater, and at South Carolina College. In 1869 LeConte moved with his family to California where he had accepted a professorship of geology at the new University of California at Berkeley. Here he lived and taught until his death in 1901.

Although LeConte made useful contributions to science, particularly in physiological optics and the geology of mountain formation, his interests were too wide ranging, his commitment to broadcasting the gospel of science too strong, to permit him to attain mastery of a specialized field in the manner of

modern scientists. He represents that phase in the professionalization of science when it was still possible to win prominence through what one of his colleagues called "generalization and philosophic inference," rather than technical expertise. Indeed, perhaps the most interesting aspect of LeConte's career is his role in gentling evolution to the service both of religion and social progress. Himself a rather slow convert to the theory of evolution, LeConte had become, by the mid-1870s, an enthusiastic evolutionist of the Lamarckian persuasion. He was also a man of Christian faith, albeit of a doctrinally liberal faith (he did not accept the divinity of Christ). His efforts to reconcile evolution and religion culminated in the publication in 1888 of *Evolution and Its Relation to Religious Thought*, a popular and influential work. Persistently optimistic about the meaning of evolution for society, LeConte, like the sociologist Lester Frank Ward with whom he corresponded, stressed the disjunction between organic and human evolution. Organic evolution might proceed through factors like natural selection, use and disuse of organs, and "the pressure of a changing environment," but humanity could cooperate in its own evolution in a conscious, purposeful way. Here the "law of love" replaced the "law of force."

Stephens's prose, though lucid, is sometimes a bit awkward, almost archaic: "the hapless fellow" (p. 57), "the aging fellow" (p. 213). A few times it is simply incorrect: "taxes were still payable, and Uncle William reported to his brothers that he had paid it out of his own pocket" (p. 95). I do not always agree with Stephens's interpretation of evolutionary theory, for example, his suggestion that because LeConte was a Neo-Lamarckian "he believed in the necessity of manipulating evolution along progressive lines" (p. 250). No doubt he did, but so did a great many staunch Darwinians like Francis Galton and the eugenicists. Shaping the future was not reserved to Lamarckians. These reservations do not seriously affect the usefulness of this volume, delineating the career of a man whose great work was to ease the transition to the evolutionary era for a great many nineteenth-century Americans.

CYNTHIA EAGLE RUSSETT
Yale University

VALERIA GENNARO LERDA. *Il Populismo americano: Movimenti radicali di protesta agraria nella seconda metà dell'800*, (Studi Storici, number 1.) Genoa: Mondini e Siccardi. 1981. Pp. 665.

Valeria Gennaro Lerda, who teaches history at the University of Genoa, has written one of the few comprehensive histories of American Populism in any language. The only others that go beyond single

states, sections, or subtopics are John D. Hicks's *The Populist Revolt* (1931) and Lawrence Goodwyn's *Democratic Promise* (1976). Lerda's work owes surprisingly little to Goodwyn's discussion of either Southern or Western Populism, and surprisingly much to Hicks (and Solon J. Buck) on the Midwest. She admires C. Vann Woodward's writings, calling his *Tom Watson* a "bellissima biografia." But she also examines Southern Populism through Antonio Gramsci's concept of the "historic bloc," in this case an "agrarian-industrial block" exercising class hegemony in the post-Reconstruction South. Her colleague and former teacher, Raimondo Luraghi, earlier applied Gramsci's ideas to the wartime and postwar South, but Lerda extends them to Southern Populism and the 1890s. This is the freshest feature of the book, and one might expect that present-day Italian scholars, many of them Gramscians, would begin to make very original contributions by applying his ideas to American topics. The book does not, however, apply the Gramscian analysis to Midwestern Populism, but instead relies on the familiar Buck-Hicks interpretation of agrarian protest. As a result, the latter stages of the book are very predictable.

Lerda spent more than two years in the United States, almost all of that time in the South. She refers to more than two dozen manuscript collections, nearly all of Southern leaders, and as many newspapers. Her familiarity with the secondary literature up to 1980, although not flawless, is very thorough. The book is copiously footnoted. Her experience probably accounts for her much more original treatment of the South than of the Midwest, and it might have helped if she had won another Fulbright and spent it in Topeka or Lincoln. It is also unfortunate that she does not discuss mountain-state Populism.

The book includes four large parts: "The South after the Civil War," chiefly about re-creating as much as possible of the prewar plantation economy, and about economic classes and "the exploitation of the black and white peasant masses" (*masse contadine*); "The West, Land of Hope," organized not around class analysis but around farmer organizations (Granges, Greenbackism, the Farmers' Alliances); "The South and West Launch the Challenge of the Third Party," a narrative of national efforts from 1888 to the Omaha convention; and "A Failed Revolution," carrying the narrative through the 1896 election. Except in the first part, the Gramscian framework is not evident except for an occasional reference to, for example, "proletarian" reform. Midwestern Populism was rooted not in class struggle but in "the egalitarian Jeffersonian tradition" (p. 174). She is aware of Goodwyn's writings and cites his book on certain points of fact, but does not follow him on controverted issues such

as his excommunication of Nebraska and the Rockies, the centrality of a "movement culture," or the cooperative commonwealth as an alternative to industrial capitalism. To Lerda, Populism sought "capitalism with a human face" (p. 589), and responded "to the needs of a middle class of small cultivators and landowners" (p. 595). It also attempted, but in the last analysis failed, she says, to unite whites and blacks.

The author shows much promise as an Americanist. Perhaps her future work might include extending Gramsci's ideas to a problem such as Progressivism, for which they might be especially appropriate, or she might compare explicitly southern Italy and the South of the United States, something only tantalizingly glimpsed in this book.

WALTER NUGENT
Indiana University,
Bloomington

JOHN L. THOMAS. *Alternative America: Henry George, Edward Bellamy, Henry Demarest Lloyd, and the Adversary Tradition*. Cambridge: Belknap Press of Harvard University Press. 1983. Pp. viii, 399. \$25.00.

This book is a successful and highly interesting experiment in the difficult form of collective biography. It deals with the three major utopian radicals of late nineteenth-century America—Henry George, Edward Bellamy, and Henry Demarest Lloyd. It is vigorously written, subtly organized, and well informed. John L. Thomas admires all three men without ignoring their faults.

Thomas handles successfully the most difficult problem of the form he has chosen: providing sufficient unity without ignoring individual differences. His three protagonists are individuals with quite different personalities and on some matters opposite opinions. Under the stress of the apocalyptic events of their day—the strikes of 1877, the Haymarket bomb and hangings, the economic crisis of the nineties, and the rise of Populism—two moved left and one right. Bellamy adopted and then qualified his authoritarian socialism; Lloyd moved from antitrust muckraking through a vision of cooperative communities to the idealistic right edge of the Socialist party. George, the first of the three to renounce classical economic dogma and the one most deeply acquainted with poverty, lived to become an embittered defender of his single-tax panacea against socialists and trade unionists.

It is quite clear, however, that the three biographies make a single, viable, and important subject. All three reformers came from marginal, insecure middle-class backgrounds and showed the traces of this origin in their personal hopes and fears. All rejected contemporary industrial capitalism on the

basis of values drawn from the same earlier traditions: the most optimistic and least skeptical parts of the Enlightenment; a Protestantism ultra-liberal in theology but still perfectionist and millennial; and the artisan-producer tradition of the Jacksonian era. All, despite their different social prescriptions, centered their hopes on the dawn of a new proto-Christian religion of humanity. They are alike also in their cycle of temporary popularity and final failure. Perhaps the book's most moving chapter deals with the decline, near despair, and death of all three, unable after the Populist collapse of 1896 to come to terms with either triumphant conservatism or new, less idealistic kinds of radicalism. The book can legitimately be read as a moving exercise in historical pathos.

In a suggestive, but much too brief, concluding chapter the author traces the later history of the ideas of his three prophets and suggests that these may after all have a future. After enjoying considerable influence during the Progressive Era, the three lost influence during the conservative and skeptical twenties. Thomas sees some revival in the thirties, especially in the period's interest in regional planning and cooperative communities, and particularly in the TVA, whose first director was Bellamy's devoted biographer. In the prosperous and confident years after World War II the three seemed irrelevant and quaint. Now, however, Thomas cautiously suggests, "the malfunctioning of Progressive bureaucracy, a new merger movement of unprecedented proportions, and the call for the dispersion of political power may suggest the possibility of reopening" this American third way.

Some professional historians may object to the minimal scholarly apparatus offered by this book. Thomas provides only a very brief list of biographies of his three figures, and refers hardly at all to historians of many topics in economic, social, and intellectual history on whom his work necessarily depends. I suspect that this is part of an effort to reach an audience beyond the guild, and at least it is a relief to read a book free of the obsessive historiographical polemic that mars too many monographs. It is to be hoped that this vigorous and skillful work reaches the wide public it serves. In any case it stands as the best account of an important chapter of American social thought.

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MARGARET S. MARSH. *Anarchist Women, 1870–1920*. (American Civilization.) Philadelphia: Temple University Press. 1981. Pp. vi, 214, \$22.50.

Although a small library on anarchism has developed since the 1960s, including monographs, re-

prints of memoirs, and two fine biographies of individual anarchist women, no volume before this one has specifically addressed ideological problems females in this movement had to confront. This book is about the conflict of ideologies within American anarchism; it is not a compendium of short biographies as the title may suggest. Margaret S. Marsh did select six late nineteenth-century women with whom to initiate her discussion, following an opening chapter placing anarchism in the American social context. Much further along attention is given those outstanding women who first come to mind when one thinks of "anarchist women," Voltairine de Cleyre and Emma Goldman. But the book concerns ideas rather than lives.

These women faced a major contradiction—the discrepancy between the anarchists' professions of principle and some anarchists' convictions about a woman's place. Men in the movement, whether "individualist-anarchists" or "communist-anarchists," while ostensibly giving allegiance to its basic premise, the essential autonomy of the individual, "absolute freedom, limited only by a prohibition against infringing on the liberties of others" (p. 10), were unable to go beyond Victorian concepts of womanhood. Even in their constructions of the coming anarchist utopia, leaders and editors such as Richard Tucker and Victor Yarros envisioned women performing the customary domestic duties of home keeping and child rearing, economically dependent on men, and leaving management of society's affairs to men. Indeed, anarchism was "an aggressively masculine world" (p. 110) that supplied no room for women except as helpmeets. Anarchist women knew very well that personal autonomy for them could only come through economic independence.

Particularly illuminating is the chapter on sexuality and reproduction, containing a detailed examination of "free love" in theory and practice. This is an idea often associated with anarchist life and thought, but however outspoken male anarchists appeared, and they did talk a bit about free love liberating women from matrimonial bondage, they proved unable to follow their rhetoric to its logical conclusion, the abolition of traditional domestic relations. They viewed the sexual drive of woman not as purely erotic, but as a compelling, instinctive desire for maternity, to realize which must forever make her—the child bearer and nurturer—an economic dependent. Moreover, some of these men condemned mechanical contraception as an evil to be shunned. In Yarros's ideal society, for instance, without private property or a money economy, members could enjoy the blessings of large families. Anarchist women knew very well, of course, that the sexual drive and reproduction were distinct. For them, voluntary motherhood through birth control

was mandatory for true self-sufficiency, which alone could deliver the personal autonomy anarchism professed. Given the fundamentally opposed viewpoints held by the two sexes in this radical movement, there can be little wonder anarchist women had a hard time of it. Not only did their lives border on the tragic, but, Marsh concludes, anarchist women never successfully integrated their pejorative analysis of the traditional domestic relationship into revolutionary theory in the anarchist framework.

The author is adept at analysis and comparison of ideas, and she has portrayed poignantly the dilemma of anarchist women. An introductory chapter on the European anarchist tradition, however, followed by a systematic effort to place American anarchist ideas within that tradition would have been helpful to the reader, especially since the book is really an intellectual history. Little was said about European antecedents, even though close links existed, such as the affinity between Jean Jacques Rousseau's educational theories in *Emile* and American anarchists' designs for their "modern School" in Stelton, New Jersey. Pierre-Joseph Proudhon and Peter Kropotkin received scant mention, Michael Bakunin and Georges Sorel none at all. There is a fine chapter, however, comparing American socialist attitudes, both De Leonist and Debsian, toward women with their anarchist counterpart. One finds a few factual errors. The author referred to Frederick Engels's American disciple, anthropologist Lewis Henry Morgan, as "Edward Morgan," for example. Even so, this book is a valuable addition to the history of American radicalism because its theme is so important, pointing up as it does one of the movement's chief ideological inconsistencies and explaining how the faithful found ways, or failed to find ways, in their own lives to accommodate this contradiction. Previous historians of anarchism have quite simply overlooked the problems generated by sexism.

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MARC D. ANGEL. *La America: The Sephardic Experience in the United States*. Philadelphia: Jewish Publication Society of America. 1982. Pp. x, 220. \$15.95.

The history of the some thirty-thousand Levantine Jews who migrated to these shores between 1890 and World War I offers many remarkable parallels to the now well-documented saga of East European Jewish settlements in this country. Like their Russian-Polish coreligionists, these Jews from Turkey, Greece, Syria, and Mediterranean environs fled to America to escape, among other political, social, and religious problems, the threat of military conscription. They experienced similar difficulties in over-

coming language barriers, in communicating with patronizing, established American Jewish organizations, in retaining their hold over their rapidly acculturating children, and in fulfilling their perception of America's economic promise. And, not unlike their fellow new Jewish immigrants, they responded institutionally to these crises of adjustment through mutual-aid societies, synagogues linking Jews from the same hometown for liturgical and social communication, and they conceived but failed to build an enduring community-wide structure in voluntaristic America. Quite unlike their East European brethren, however, until now little scholarly attention has been directed their way.

The story of Levantine Jewry in America has been held back because Sephardim, even more than their Russian Jewish fellows, were very poor record keepers and preservers. More important, because of their relatively limited numbers, they were apparently "invisible" both to the government officials and Progressive reformers who wrote the reports and tracts that provide much of the essential external documentation used in recounting the Yiddish-speaking downtowners' lives. Finally, their foreign-language press, the internal source so crucial to any study of immigrant progress in America, is, in the Levantines' case, limited to a number of weekly journals that hardly flourished during the era of arrival, and were published in Judeo-Spanish, which few historians in America are trained to use.

Accordingly, the very appearance of Marc D. Angel's study of the Sephardic experience in America represents a distinct historiographic contribution. This American-born grandson of Turkish immigrants to the United States, fluent in Judeo-Spanish, has dutifully written a chronicle of his ancestors' migration and settlement patterns and their institutional life as seen from the pages of *La America*, the first Judeo-Spanish weekly newspaper published in New York between 1910 and 1925, and through the eyes of its editor, Moise Gadol. Working almost exclusively from that one extant source, Angel succeeds in identifying and classifying scores of immigrant organizations. Economic progress is considered primarily through the success stories recorded in the newspaper. And those communal disputes and disagreements that gained popular attention are discussed in great detail. Indeed, Angel seems compelled in those areas where information has survived to present the reader with the totality of his sometimes undigested research, often obscuring the conceptual point under consideration. He also often fails to remind us that the point of view of one highly partisan foreign-language newspaper does not constitute the outlook of the entire community it sought to serve. Such are but two of the pitfalls confronting the historian seeking to write a comprehensive communal history based

on the most limited and parochial of sources. Still in all, this work is welcomed and one hopes will inspire both its author and other like-minded scholars to continue the examination of this most understudied segment of the Jewish immigrant saga.

JEFFREY S. GUROCK
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VERONICA E. VELARDE TILLER. *The Jicarilla Apache Tribe: A History, 1846-1970*. Lincoln: University of Nebraska Press. 1983. Pp. viii, 265.

As Vine Deloria, Jr., has observed, the studies of Indian history most needed are of the post-reservation period. About two-thirds of Veronica E. Velarde Tiller's solid and readable study concerns this critical era. Although a Jicarilla Apache herself, Tiller does not claim to write from the Indian point of view for she relies on government documents and studies by non-Indians. Her views of recent Jicarilla history, however, are certainly based on more than published works.

In 1974 Dolores Gunnerson's *The Jicarilla Apaches: A Study in Survival* brought the tribe's history down to 1801. Tiller tells their story from the coming of the Americans to 1970, covering even their bleakest years with scholarly detachment.

The Jicarillas asked repeatedly for a reservation in their own lands, but the government yielded to civilian pressure and ignored them. In 1887, after refusing to remain at Mescalero, they were finally given a reservation in northern New Mexico. Settlers claimed the only part of it suitable for farming; the rest was rangeland, but only a few Indian families owned sheep or cattle, and none was provided. They were expected to farm.

In the decades after 1900 government policies and neglect drove the Jicarillas to the brink of extinction. In 1907, ironically, their reservation was doubled in size while others were being reduced. With substantial grazing land, they owned at most eight thousand sheep and one thousand cattle. Logging began on the reservation in 1908, but tribal income was placed in the U.S. Treasury. In 1914 and 1915 a tribal flock of sheep and a cattle herd were purchased with a reimbursable loan, but again any profits were held by the Treasury.

In this period the Jicarilla population declined from 815 to 588. Seventy-five percent suffered from tuberculosis, and it was predicted that they would soon disappear. Diseases, extreme poverty, and a high death rate produced profound apathy. In 1920 agent Charles E. Faris introduced plans for health rehabilitation and distributed sheep to families. Tuberculosis was gradually checked—by 1940 health was no longer a problem. The spread of sheep

raising had a positive effect, and the Jicarillas rose from the depths of despair. All of this was inexcusably long overdue; but for the Jicarillas' innate survival ability, it would have been too late.

Since the 1930s the Jicarillas' situation has steadily improved. Oil and gas leases have added to tribal income. A tribal scholarship fund enables any Jicarilla to pay for college or vocational training. In 1970 a favorable decision in their land suit, after years of litigation, provided them with funds for improving the reservation and for starting new industries. Through their own efforts the Jicarilla Apaches have taken long strides toward self-determination, but they still have far to go. Not only students and scholars but also citizens who are concerned about the condition of the tribes as well as those who suspect that the government pampers Indians should read this excellent book.

DONALD E. WORCESTER
Texas Christian University

GERALD P. FOGARTY. *The Vatican and the American Hierarchy from 1870 to 1965*. (Päpste und Papsttum, number 21.) Stuttgart: Anton Hiersemann. 1982. Pp. xxi, 438. DM 180.

Imagine a history of smoke-filled rooms at the Democratic National Convention. Men rush by; cabals form and disintegrate; strategems rise and fall. Simultaneously, the ship of state plows on. The connection between the Byzantine dealings of power brokers and the functioning of the polity at times seems tenuous. But out of the rooms emerge the Willkies, Eisenhowers, and Kennedys.

Such thoughts emerge as one reads Gerald P. Fogarty's impressive history of the American hierarchy in its dealings with the Vatican from 1870 to 1965, from the papal infallibility of Vatican I to the declaration of religious liberty of Vatican II. Rather than trace the history of American Catholics, Fogarty treats the dealings of a hierarchy that has set the mode in which Catholics practice their faith in the 1980s.

The author has several themes, but the most important is the maturation and independence of the American hierarchy. Refusing to recognize the ethnic diversity of the American church, the secular nature of public schools, and the pragmatic virtues of religious toleration, the Vatican by 1900 had imposed a Roman-trained leadership and a siege outlook on a hierarchy once noted for its collegiality and practicality.

World War II, which saw the rise of American globalism, allowed American bishops to throw off this thralldom. In the postwar world the American hierarchy played a direct role in helping the Vatican save Italy from a communist government. Allen

Dulles helped channel funds to the Vatican to rebuild war damaged property. The anticommunist crusade provided the occasion for cooperation, and the election of John Kennedy convinced the Vatican that pluralism might have merits.

With the pontificate of John XXIII and the presidency of John Kennedy the scene was set for the emergence of the American hierarchy. In the preceding years the Vatican had revealed gross ignorance of the American experience. Nowhere was this ignorance more pronounced than on the issue of religious freedom. The orthodox position at Rome resembled the medieval idea of one king, one church. But with the writings of John Courtney Murray and the sponsorship of Cardinal Spellman and other American bishops, the New World church, despite faulty Latin and an anti-intellectual reputation, began educating the old. The American hierarchy gave the Vatican Council something more valuable than subtle theological distinctions. Eventually the papacy accepted the pluralistic approach to religious liberty.

Fogarty sketches this story in elaborate detail. Sometimes the figures flit before the reader at speeds that defy memory and in characters that resemble Tammany politicians and Renaissance princes. Yet the dedicated reader is rewarded for plowing through the plots and counterplots. The author has unearthed an impressive amount of primary evidence in the Vatican and various diocesan archives, most of which have never been seen before by scholars. Students of American history as well as church history will benefit from this book.

GEORGE Q. FLYNN
Texas Tech University

LEWIS GREEN. *The Boundary Hunters: Surveying the 141st Meridian and the Alaska Panhandle*. Vancouver: University of British Columbia Press. 1982. Pp. xii, 214.

When the United States acquired Russian America in 1867, President Andrew Johnson's administration accepted the boundary defined in the 1825 Anglo-Russian treaty. Nevertheless, the United States and Canada soon announced differing interpretations of the vague line stretching northward from the southern tip of Alaska's panhandle (southeastern Alaska). The matter compelled diplomatic resolution, especially after the rush of gold seekers at the turn of the century. Only after numerous and protracted negotiations was the geopolitical line ultimately settled; and not until 1920 was the actual border fully marked. Although the dispute never seriously produced war fever, it muddled U.S.-Canadian relations for decades.

Author Lewis Green, a British Columbia geolo-

gist, has written an engrossing and carefully balanced account of this phase of Far North history. As his title reveals, Green gives particular emphasis to the surveyors who marked and fixed the geographic boundary. Given the hazardous terrain in which they labored, neither the Canadian nor American fieldworkers had time to worry over political "huff and puff" generated in London, Ottawa, or Washington.

Green's work is divided into seven chapters: 1825–67, British-Russian boundary relationships; 1869–96, the U.S. and Canadian marking of the 141st meridian; 1876–96, conflicting panhandle claims; 1896–1903, the impact of the Klondike gold rush; 1903, the Alaska Boundary tribunal climaxed by President Theodore Roosevelt's threat; 1904–20, the transposing of paper map lines into aluminum-bronze monuments in the panhandle; and the final chapter, the similar undertaking along the 141st meridian.

In his afterword, Green reminds us how perverse history can sometimes be. Since 1977, new United States–Canadian disagreements have emerged "as both countries sought to extend their boundaries seaward to a new two-hundred-mile limit." In this instance, however, "There is no help from the past; neither the 1825 treaty nor the 1903 Award offer any guidance as to where the new boundaries should run" (p. 178).

Along with his quite helpful bibliography and extensive citation are the book's excellent photographs of the surveyors and their equipment. Geographers may quibble that the maps are inadequate, however, historians will doubtless find them satisfactory. Alaska historians are sure to appreciate the appended translation of the original 1825 Anglo-Russian treaty. Anyone seriously pondering United States boundary and diplomatic controversies with Canada must examine Green's *Boundary Hunters*.

TED C. HINCKLEY
San Jose State University

GALEN CRANZ. *The Politics of Park Design: A History of Urban Parks in America*. Cambridge: MIT Press. 1982. Pp. xiii, 347. \$25.00.

Far from being "just there like the weather," parks have both a history and a political role—or several. This interesting study deals first with the historical role of park development and use in the United States from 1850 to the present. Usage and design fall into four periods: the pleasure ground, 1850–1900; the reform park, 1900–30; the recreation facility, 1930–65; and the open space system since 1965. In each period, as Cranz shows by quotations from the records of particular American cities, the aims and patronage of parks differed. These quota-

tions and references to particular situations are a strength of the book. On the other hand, the necessity of turning repeatedly from one example to another contributes to the rather jerky style, which sometimes reads like an outline from which the author has had to remove transitional phrases and leisurely descriptions. Each section is illustrated with well-chosen and pertinent views and maps.

The second part of the book discusses "the politics of park design." Those with power to make decisions about parks are "idealists, philanthropists, commissioners, professionals, and bureaucrats." Those who use the parks have sometimes been grouped by class and sometimes by role: "families, children, adolescents, the elderly and handicapped, ethnic and racial groups, women." The kinds of benefits expected from building parks have varied from increased status for the city to tourist revenue, increased health, decreased social tension, and so on. Who assessed benefits and who received them has varied as much as any other political reality connected with expenditure of public money. Equally diverse have been the roles of parks, from social control to aesthetic pleasure.

In making us conscious of the many ways that parks reflect and influence society, Cranz has performed a useful service. One hopes that others will develop in greater detail some of the issues (such as assessment) raised here, and that Cranz himself will turn his sociological training and architectural setting (University of California, Berkeley) to further advantage in continuing exploration of urban infrastructure.

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STUART CREIGHTON MILLER. *"Benevolent Assimilation": The American Conquest of the Philippines, 1899–1903*. New Haven: Yale University Press. 1982. Pp. xii, 340. \$25.00.

Readers of this book may not know precisely why it was written even after turning the last page. Nonetheless, they should welcome this newest entry in the historiography of American imperialism in the Philippines.

Stuart C. Miller is not the sort of author who uses time and space in explaining his purposes. His work most closely resembles Richard E. Welch, Jr.'s *Response to Imperialism: The United States and the Philippine-American War, 1899–1902* (1979). Both relate why McKinley chose to annex the islands, how the anti-imperialists resisted his choice, and how the army enforced it by suppressing the Philippine Insurrection, or, as Miller prefers, "war." Neither book offers anything original about McKinley's decision to annex the Philippines. Both are excellent in

their accounts of anti-imperialism and the war in the islands. Welch excels in the former and does not pretend to be indispensable on the military story; Miller, on the other hand, eventually concentrates on the war, America's first guerrilla campaign in Asia.

He does devote an entire chapter to the subject of anti-imperialism, and it is a high point of the book. Miller takes good advantage of existing historiography, generously offering due credit. He caustically portrays anti-imperialists as self-righteous egotists with a talent for self-destruction. He also makes the intriguing suggestion that, although "paternalistic" racists usually ended up in the camp of annexationists, the worst "race haters" became anti-imperialists (p. 123).

Miller's focus, however, is on the cruelty of the U.S. Army in the Philippines and the search for alibis at home. No lingering doubts should survive that American forces committed numerous atrocities in this war. Moreover, torturing prisoners, killing civilians and children, destroying life-supporting crops, and burning villages to the ground were dictated by official policy. American troops hated their foes, and most reveled in killing them wholesale. "The country won't be pacified," said one soldier, "until the niggers are killed off like the Indians" (p. 179). Apart from a few token court-martial, this savagery found acceptance at home: President Roosevelt and Secretary of War Elihu Root stonewalled on the facts and attacked critics as traitors; some of the worst offenders became army chiefs of staff under T. R.; and the press, oddly just as "muckraking" of domestic issues became popular, docilely rolled over after a few sharp barks. The fizzling out of criticism, the author concludes, conspired "to protect American innocence" (p. 260).

"Benevolent Assimilation" is not a faultless work. Miller keeps Carl Schurz in the Senate a generation after his departure. Miller's rhetoric might cause readers to believe that Filipino casualty figures were far greater than they actually were (estimated by Welch as sixteen to twenty thousand killed in combat and two hundred thousand from hunger and other causes). Some of the declarations about traditional American views of race, mission, and innocence are undeveloped and often seem half-hearted. These are minor flaws, however, in a forceful, thoroughly interesting book.

ROBERT L. BEISNER
American University

ALFRED E. CORNEBISE. *Typhus and Doughboys: The American Polish Typhus Relief Expedition, 1919-1921*. Newark: University of Delaware Press or Associated University Presses, East Brunswick, N.J. 1982. Pp. 188. \$24.50.

I hate the kind of review that starts "Mr. Melville has written a book about an obsessive ship's captain and a white whale when what the world really needs is another *Madam Bovary*." Authors should be judged on how well they achieve what they intend, not on their success with what the reviewer wishes they had intended. Alfred W. Cornebise's *Typhus and Doughboys* is the story of the American soldiers who went to Poland after World War I to delouse as many Poles as they could. It is not about the Bolshevik Revolution, the origins of modern Poland, the Russian Civil War, the Russo-Polish War, or even about the typhus epidemic as such. These are barely referred to. Lenin is mentioned zero times, Piłsudski three times, and Wilson six times. Colonel Harry L. Gilchrist, Commanding Officer of the American Polish Typhus Relief Expedition, is mentioned eighty-one times. Typhus, oddly enough, rates only eighteen mentions, is never described in any detail, and the extent of its awful ravages only implied. *Doctor Zhivago* tells us a lot more about the impact of typhus than this book. We do learn, however, on page 88 that Major Willis P. Baker arrived in Warsaw in August of 1919, soon moved to the Hotel Versal in Kielce, and thence to Krakow and Oswiecim in November. And we learn on page 99 that one must seal all the cracks in a barracks to effectively disinfect it with poison gas, for which purpose the Americans initially used hydrolized starch derived from horse chestnuts. That proved to be too expensive, so they switched to a local clay "with a certain admixture of horse manure."

Typhus and Doughboys is derived from an extensive review of materials in the National Archives and from a handful of secondary sources, all in English. The book tells us all we will ever want to know about the doughboys who went to Poland to fight a deadly epidemic in the midst of a society in violent disorder, but it tells us little about that society. It does note that Bolshevik POWs were among the very few allies the Americans found in their battle with disease, a tiny detail which may have a special resonance for historians of the larger movements of the period. *Typhus and Doughboys* is not a major work and is not intended as such. It is a well-researched bit of military antiquarianism, and may provide a detail or two for major studies of the political, military, and epidemiological chaos that reigned in Eastern Europe immediately following World War I.

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ROBERT SAMUEL SUMMERS. *Instrumentalism and American Legal Theory*. Ithaca: Cornell University Press, 1982. Pp. 295. \$24.50.

This is essentially a discourse on legal philosophy, Robert Samuel Summers stresses, and is "*not primarily a historical study*" (italics in original, p. 12). An analysis of the published writings of several important American jurisprudential thinkers is the core of the work. Oliver Wendell Holmes, Jr., Roscoe Pound, Karl N. Llewellyn, Felix S. Cohen, Jerome Frank, and John Dewey comprise the better-known figures considered, but also given attention are less familiar, though nonetheless notable, individuals, including John C. Gray, Walter Wheeler Cook, Joseph Walter Bingham, W. Underhill Moore, and Herman Oliphant.

During the first half of the twentieth century the ideas of these men contributed to a transformation in legal thought from formalism to what the author calls pragmatic instrumentalism. Although not in and of itself a general theory of law, formalism did embody certain connected presuppositions. Law was thought to be value neutral, it supposedly derived from pre-existing principles found in statute and case books that could be discovered through deductive logic. These presuppositions fostered the conviction that courts discovered rather than made law. By contrast pragmatic instrumentalism may be understood as a general theory according to which law is conceived of as "action oriented," fully capable of achieving substantive social purposes that shape society. This theory assumes that courts make law. As a philosophical inquiry, I think, this book succeeds; it is always thought provoking and is usually convincing. The author makes a strong case that pragmatic instrumentalism comprises a distinctly American contribution that should be ranked along with natural law, British and Austrian analytical positivism, and historical jurisprudence as the West's major contributions to legal thought.

As history, however, the study is weak. This weakness would be incidental if it did not bear upon the author's overall purpose, but such is not the case. Formalism is identified with political conservatism, laissez-faire economics, and biological determinism with its "fatalistic implications." Pragmatic instrumentalism is presented as growing out of Progressivism, relating especially to assumptions about the ability of technology and social science to change society. The problem with this conceptualization of turn-of-the-century ideas and policies is that it is dated. Herman Belz, Charles McCurdy, Stanley P. Caine, Ellis Hawley, Barry Karl, Morton Keller, Henry May, Edward Purcell, and many others have pushed our understanding of Progressivism and laissez faire far beyond the simple dichotomies presented here.

One example may illustrate the problem. The book presents Joseph H. Beale, the founder of the legal field of conflicts of law, as an arch formalist. Yet a recent study of Beale's use of *Swift v. Tyson* in

the development of his conflicts theory shows that he considered himself a patron of historical jurisprudence. Consistent with this outlook, what was Beale's view of law? In his private papers and more obscure published writings he defines law in much the same terms as the pragmatic instrumentalists are supposed to have thought of it: as "a tool, with which society works out its activities." No doubt a fuller treatment of the historical context and continuities that underpinned both formalism and instrumentalism would make possible a reconciliation of Beale's ideas with the basic argument of this book. Such a treatment would have an impact beyond the circle of those concerned with the dynamics of legal theory alone.

TONY FREYER

University of Alabama

NICK SALVATORE. *Eugene V. Debs: Citizen and Socialist*. (Working Class in American History.) Urbana: University of Illinois Press. 1982. Pp. xiv, 437. \$24.95.

In 1949 Ray Ginger concluded his masterful biography of Eugene Victor Debs (*The Bending Cross*, p. 459) with the following trenchant observation: "the story of Eugene Debs becomes the story of a whole generation of wage earners and dirt farmers." Thirty-three years later Nick Salvatore's new biography of Debs adds further meaning and depth to Ginger's observation. In this stunning book, Salvatore sets Debs firmly within the central traditions of United States political and social history and depicts, as never before, the triumph and tragedy that characterized the socialist leader's personal and public life. He also explores with a deft hand the travails of the first generation of American radicals who had to grapple with the emergence of modern corporate capitalism.

In the years between the two biographies Debs's historical reputation fell on hard times. Scholars neglected him or dismissed him as all radical heart and little intellect. Others decried his failure to participate more fully in Socialist party internal affairs, especially at moments of crisis. And still worse indictments followed. In 1971 Richard Jensen (*The Winning of the Midwest*, p. 264) castigated Debs for his role in the 1894 Pullman Strike, and ten years later Aileen Kraditor (*The Radical Persuasion*, p. 149) dismissed Debs as a person with "a mind out of touch with reality." Those who now read Salvatore's biography will not recognize Jensen's or Kraditor's Debs.

It was hard for me to imagine that either the quality or style of Ginger's biography could be improved. But Salvatore has done just that. Ginger's prose still sparkles yet Salvatore also has a way with language. He writes with warmth, grace, power, and

feeling. *Eugene V. Debs: Citizen and Socialist* moves, and moves the reader. More important, Salvatore surpasses Ginger in scholarship. He provides the documentation and annotation absent from *The Bending Cross*, and he never allows literary license or dramatic necessity to shape the story. Moreover, Salvatore uses sources unavailable to Ginger as well as benefiting from the publication explosion in social and labor history during the 1960s and 1970s. This book is truly a labor of love, love that drove the author to seek out every source and fragment, which might more clearly illuminate Debs's life.

If the life we follow does not differ greatly in its broad dimensions from that sketched by Ginger, it is nevertheless portrayed with a surer and fuller touch. Salvatore explains how such conventional American values as republicanism, equality of opportunity, and evangelical Protestantism carried Debs from his career as a small-town, trade-union Democrat to his ultimate role as an advocate of Marxian Socialism. We follow the life of a man who in 1884 declared labor and capital allies and opposed strikes. Only ten years later Debs committed himself to the class struggle and suffered imprisonment as a result of his leadership in the Pullman Strike. Then, in 1905, he helped create the IWW. We comprehend how in his days as a conventional trade unionist, Debs "discovered that his culture's Protestant religious imagery was particularly suited to both his emerging new message and to his public personality. In the patriarchs of the Old Testament and in the angry Christ of the New, Debs found a prophetic model that legitimized his critique" (p. 64). As he developed from craft unionist to industrial unionist to Populist to socialist, Debs never slighted themes drawn from Christianity. "What is Socialism?" he asked at a speech in Newark, New Jersey, in 1898. He answered: "Merely Christianity in action. It recognizes the equality in men" (p. 165).

Nurtured in the spirit of middle American republicanism and Protestantism, Debs, no matter how far he drifted to the left, could never distance himself from the democratic ethos. In his final years a tired, frail, and ailing old man, Debs was forced to choose between new Communist friends and old socialist comrades. An early admirer of the Bolshevik Revolution and the recipient of encomia from Lenin, Debs chose the "American Way." For him the Communists were mistaken in their commitment to a nonindigenous ideology and in their contempt for "bourgeois democracy." For him, the Socialist Party of America, weak and frail in the years 1922–24, still typified that which was best, most humane, and most democratic in the American tradition.

Just as Salvatore brilliantly illuminates Debs's public career, so he enlarges our understanding of his private life. Although not a psychological study in any formal or conscious manner, the book captures

its subject's deepest emotional crises and his inability to resolve them. We see a man at emotional war with himself, torn between the ideal of the respectable Victorian family and his own emerging radicalism, a man who extolled his mother as a paragon of the cult of domesticity and then waged war against bourgeois gentility. We glimpse a man who endured a dreadful marriage to a woman who wanted wealth, status, and respectability. Yet he maintained his formal fidelity in the face of real anguish. We see a Debs who fell deeply in love with another woman (Mabel Dunlap Curry). Between 1916 and his death in 1925, he could never sublimate his need for Mabel and yet could also never bring himself to consummate the relationship. Mabel Curry appears only once, in passing, in Ginger's 459-page biography. Salvatore uses what remains of the Debs-Curry correspondence to show us how important Mabel was to Eugene and to disclose hitherto hidden aspects of Debs's personal life and personality.

In the end, Debs's public career proved as tragic as his private life. The ultimate irony for Debs was that a man so encapsulated within indigenous American political and cultural traditions could never sway more than a small minority of fellow citizens to believe in his vision of a more democratic, more egalitarian future. As Salvatore remarks, Debs and other American socialists never failed, for "Failure assumes the possibility of success, but that was never a serious prospect for the Debsian movement" (p. 271). The same American traditions—republicanism, evangelical Protestantism, and equality of opportunity—that brought Debs to socialism motivated the vast majority of American workers to remain Republicans or Democrats.

A few typographical and spelling errors notwithstanding, this well-designed, handsomely produced, and amply illustrated book deserves to be read and treasured as the finest life of a modern American radical now in print. It brings great distinction to the University of Illinois Press series, *The Working Class in American History*, and to its author Nick Salvatore. The Bancroft Prize Committee agreed.

MELVYN DUBOFSKY
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WILLIAM L. O'NEILL. *A Better World: The Great Schism, Stalinism and the American Intellectuals*. New York: Simon and Schuster. 1982. Pp. 447. \$17.95.

A Better World is one of those books that is valuable both on account of its contribution to knowledge about a given era and also because the information presented about past events reaches into the present and illuminates it. Although it focuses on the de-

bates about the nature of the Soviet system under Stalin, it is also a thorough record of the startling misjudgments and misperceptions of an important and influential portion of American intellectuals between the late 1930s and the early 1950s. By extension, it is a case study of the processes of political self-deception and of the mechanisms people rely on to rationalize, prolong, salvage, or revitalize their political commitments. Moreover, the book is well written, witty, judicious, and at times appropriately judgmental.

Three groups of intellectuals are dealt with: fellow travelers, progressives, and anti-Stalinists. The dividing line between the first two is not always clear, as the author also recognizes; the fellow travelers went further in extolling the virtues of the Soviet regime and rationalizing its misconduct when it could no longer be ignored. But both progressives and fellow travelers shared highly integrated attitudes that centered on the reflexive disparagement of American society (that is to say, on alienation) and a predisposition to admire social systems such as the Soviet, on which it was possible, for a while at any rate, to project all the virtues found lacking in the United States. Selective perception, wishful thinking, selective moralizing, and double standards were essential in the evolution and maintenance of these attitudes.

The sources most extensively used in this study are *The New Republic* and *The Nation*, which for decades registered and reflected left-wing (or progressive) opinion and the disputes it generated. William L. O'Neill also makes excellent use of the personal papers and correspondence of many important figures of the period. The major examples of progressives and fellow travelers (their descendants and present-day sympathizers) include Bruce Bliven, Malcolm Cowley, Alger Hiss, Lillian Hellman, Maurice Hindus, Freda Kirchway, Owen Lattimore, Max Lerner, Corliss Lamont, F. O. Matthiessen, Victor Navasky, Frederick L. Schuman, I. F. Stone, Anna Louise Strong, Henry Wallace, and Alexander Werth. The smaller group of anti-Stalinists are represented, among others, by Louis Fisher, Sidney Hook, Irving Howe, Alfred Kazin, Dwight McDonald, Mary McCarthy, William Phillips, Arthur Schlesinger, Jr., and Diana Trilling.

The narrative traces pro-Soviet attitudes from the late 1930s through the postwar years when the reaction set in, "progressivism" declined, and McCarthyism and the black lists arose presenting new controversial issues and soul searching among liberal intellectuals and academics in particular.

The questions raised by this book and much of the evidence underlying them go beyond the issues of Stalinism and anti-Stalinism. The author presents an enduring mindset characteristic of alienated intellectuals whether or not the focus of their affection

was the Soviet Union under Stalin, or more recently, China under Mao or Castro's Cuba, or the obscure entity called the Third World. Certainly the veneration of the Soviet Union was the most egregious and emblematic of all such political infatuations, and set the pattern that continued, if in a somewhat diminished and chastened form, well into our times. These continuities are not taken up by O'Neill, and hence the impression may be created that the misreading of the nature of the Soviet Union under Stalin was more a singular error of judgment than part of a broader predisposition.

In the 1930s and 1940s, as in more recent times, what O'Neill calls "addiction to false analogy" and wishful thinking were widespread. F. O. Matthiessen (Harvard professor of literature) equated Soviet censorship with American popular culture. Carey McWilliams compared the Soviet purges to the American loyalty program. When particular Soviet misdeeds came to light (as for example the execution in 1943 of two Polish socialists, Alter and Ehrlich), they were either seen as errors incidental to the other praiseworthy policies and goals or reduced in moral significance by comparing them with allegedly greater American evils. When the sixteen Polish underground leaders invited to negotiate in Moscow after World War II were jailed, Max Lerner advised that "the big picture had to be kept in mind." I. F. Stone was even more reassuring, telling the readers of *The Nation* "immediately after the Poles were arrested that he knew enough 'to advise American progressives to keep their shirts on . . .'" Owen Lattimore (also known for his misidentification in 1944 of the prison camps of Kolima as flourishing enterprises reminiscent of the TVA and Hudson Bay Company) wrote in 1947 that "the Czechoslovak Communists, heartily backed by the Soviet Communists, are giving a thorough trial run to a policy of suggesting . . . that socialism and eventual communism can be reached by easy transition, with prosperity all along the way and without massacre or coercion."

Not only did many "progressives" and fellow travelers try to justify the purges of the 1930s, but also the purges in 1953 were dealt with in a similar fashion by *Monthly Review*. O'Neill writes: "They knew there was something rotten in the Communist East but wanted to be persuaded that they should support it even so. *Monthly Review* accomplished this in two ways. One was to redefine basic ideas such as freedom and democracy . . . enabling one to be at the same time a Stalinist and a democrat, for example. Another was to show how obvious horrors were not so bad as they seemed and the West's fault in any case." The *Monthly Review* also justified Soviet political violence and coercion by pointing out that it was "used in the interests of creating a truly human society from which it will be possible at long last to

banish violence altogether." Perhaps part of the problem then, as in more recent times, was that many intellectuals believed as did *The Nation* that "ignoring or apologizing for violations of human rights abroad does not prevent one from effectively championing them at home." Thus today, the "progressives" of the Institute of Policy Studies and other contributors to *The Nation*, such as Ronald Steel, direct their "anger . . . not at the Soviet occupation [of Afghanistan] but against those who complain of it."

It was in the period scrutinized in this volume when the foundations for much of the conventional political wisdom of our times were laid down, including the habit of dismissing left-wing repression by cautioning against "ethocentrism" and noting that we cannot expect such regimes to be democratic "according to Western standards" or in the "Western style." The belief that Marxist regimes achieved wonders in raising material standards of living, although somewhat neglectful of civil rights, has also gained wide acceptance, notwithstanding the recurrent outflow of refugees from such societies seeking to escape economic as well as political privations.

The author is somewhat hesitant in assessing the overall significance of the events and attitudes he presents, though he inclines to the belief that "history would have turned out the same. American intellectuals played minor roles." In this, I think he underestimates the long-range, cumulative effect of the attitudes (and conflicts) he so well captures. While locating "the eclipse of progressivism" in the early 1950s, he cites evidence of the continued acrimonious exchanges between the protagonists and their successors up to our times, but allows too little space for an examination of the current state of the "great schism" replete with the resurgence of anti-anti-communism and alienation in many academic-intellectual communities and subcultures. Perhaps the issues dealt with do not reduce to conflict between Stalinists and anti-Stalinists. As I see it, the "schism" might be better defined as one between those opposed to authoritarian regimes legitimated by Marxism and critical of such (and all other) dictatorships, and those who believe that material improvements justify political repression. Even more generally and significantly, the conflict exists between intellectuals alienated from and hostile toward their own society (and hence perennially susceptible to the attractions of others) and those who are basically supportive of their society and appreciative of its political and intellectual freedoms.

Whichever way one explains the misjudgments and rationalizations of the Stalinists—as resulting from ignorance, idealism, good intentions, overemphasis on ends, mixtures of deception and self-

deception—the attitudes and their vocal expression made a difference. For one thing, they debased intellectual discourse for decades and made it possible, or easier, for McCarthy's demagoguery to arise, which in turn discredited anti-Communism among intellectuals with enduring results. Conceivably, the pro-Soviet attitudes of influential intellectuals and journalists might also have contributed to a climate of opinion during and after World War II that, in turn, might have influenced American policymakers who made the sphere of influence agreements that consigned Eastern Europe to Stalin. Of course, while plausible, connections between climates of opinion (in this case favorable images of the Soviet Union) and specific foreign policies are hard to prove. And, although the stock of the Soviet regime has declined, periodic attempts to purify that much-tarnished image and revive that of a peaceful and reasonable power keep popping up, especially when the desire for disarmament is strong. But there is a more consequential heritage of the "great debate." It is the virtual institutionalization of estrangement that was, in the first place, the source of the misperception and idealization of the Soviet regime. This estrangement has survived, and it remains an obstacle to clear thinking about both the nature of American society and about the contemporary threats to political and intellectual freedoms in various parts of the world.

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MARGARET W. ROSSITER. *Women Scientists in America: Struggles and Strategies to 1940*. Baltimore: Johns Hopkins University Press. 1982. Pp. xviii, 439, \$27.50.

Margaret W. Rossiter has written a comprehensive history of American women in science to 1940. Her study is based on an astounding amount of research in reference works, periodicals, government publications, statistical studies, biographies, and, above all, manuscript collections and oral histories in over seventy archives. She uses this massive evidence to create a compelling statistical picture of every facet of the position of women in the scientific professions, a collective portrait she then illustrates with dozens of well-chosen case histories. Her book is both a major contribution to the growing literature on the history of women in the professions and the historical sociology of science.

Rossiter focuses on three areas of major concern to women with scientific aspirations: acquiring the best available education; finding employment suited to their interests, training, and talents; and winning

appropriate recognition from their peers. She begins early in the nineteenth century before science was professionalized and when women were struggling for secondary educations, and ends just before World War II, when the doctorate was the *sine qua non* for a scientific career and the present institutional structure of graduate departments, professional associations, prestigious prizes, and foundation support had emerged.

The story Rossiter tells is a discouraging one. As a group, male scientists (there were exceptions of course) simply did not want women in their fields and were remarkably successful in keeping the visible scientific world a masculine one (again with exceptions). Rossiter traces their behavior to a reflexive association between prestige and masculinity. Thus, as various fields of science were professionalized in the late nineteenth and early twentieth centuries, concern with defining and upgrading standards almost invariably meant keeping women out and marginalizing those already there. Rossiter does not refer to economic motivation, but surely, like doctors, scientists also hoped to keep their salaries higher by eliminating competition, particularly in a world where women routinely earned less than men no matter what the work involved.

Rossiter's study implicitly raises the question of whether women can ever achieve professional equality in a society characterized by gender hierarchy and the persistent deprecation of female abilities and personality. Her findings suggest a negative answer. In science women broke through one barrier after another only to find new ones in their path. When they won their battle to earn doctorates in the 1890s and early 1900s, for example, they discovered that their prized degrees did not lead to the expected careers. By World War I, a pattern of territorial and hierarchical segregation characterized employment throughout the scientific world, while professional associations created membership hierarchies with qualifications for the upper ranks that excluded most women (largely because of their limited occupational opportunities). The few women who did scale the heights owed their success to influential male patrons—and even then success came much later in their careers than would have been the case had they been men. Rossiter has proved beyond a shadow of a doubt that, contrary to their own claims and to popular belief, the scientific professions do not function as meritocracies open to talent, recognizing and rewarding achievement according to objective standards.

Rossiter ultimately leaves the reader with a powerful impression of the irresponsible social waste and individual pain and frustration caused by the patterns of discrimination she documents. One can only hope that the promised second volume of her study covering the years from 1940 to the present

shows the scientific professions finally extending a welcome to someone besides white Christian men.

BARBARA J. HARRIS
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C. L. SONNICHSEN. *Tucson: The Life and Times of an American City*. Maps by DONALD H. BUFKIN. Norman: University of Oklahoma Press. 1982. Pp. xiv, 369. \$29.95.

Senior editor of the *Journal of Arizona History* at Tucson and absentee H. Y. Benedict Professor of English, Emeritus, at the University of Texas at El Paso, C. L. Sonnichsen calls himself a "grass-roots historian." Considering his region and the hardness of his subjects—if roots are the thing—he is more honestly a "yucca-root historian." Beginning in 1942 with *Billy King's Tombstone* he has written some twenty books, all about ordinary, odd-ball people, mostly scattered over the dry landscape of west Texas, New Mexico, and Arizona.

Tucson is the latest. Opening with "Precarious Paradise" and the warning that deserts were not meant for people and closing with "Back to the Bowl and Pitcher" and the sobering fact that no other North American city of its size (about 500,000 in 1980) has to rely on groundwater alone, the book is a lively social history of the community, chronologically told, from Papago village and Spanish presidio (a token nod, no more), to racially mixed town, to Sun Belt metropolis.

As the photographs pick up—and there are lots of them, mostly of people—so does the author. He shows us how unreliable our stereotypes of greedy traders and vicious Indian agents are, and he admonishes us to judge the pioneers "by the light that was given to them" (p. 107). Only occasionally does he let his own biases show, for instance, when describing the repressive vigilante spirit condoned during World War I, "which should have gone growling back to its jungle when the armistice was signed" (p. 205), or when tipping his hat to the male lead in the 1951 movie *The Last Outpost*, much of it filmed in Tucson: "Providence mercifully hid from curious Tucsonans the destiny which was awaiting the agreeable Mr. Reagan" (p. 301).

Sonnichsen's charm and wit are present as usual, starting with the book's dedication "to all those living elsewhere who would rather be in Tucson." He keeps a close eye on the girls of Gay Alley; reports on the planning of a volunteer fire department before there was water enough to fill its buckets; chronicles the sensational crimes of the 1930s and the capture of the Dillinger gang by the TPD; balances cultural, economic, and recreational developments; and restrains himself when noting Tucson

"firsts," among them the opening in 1919 of the first municipally owned airport in the United States.

Perhaps the theme of Tucson's isolation is overplayed. Although the author handily sees the community through national ups and downs—"No city in the Nation handled the depression better" (p. 230)—there is little effort to link it to, or contrast it with, Albuquerque, El Paso, Phoenix, or Hermosillo to set it in the context of evolving regional geography as suggested by D. W. Meinig's *Southwest*.

At base *Tucson* is the book for those thousands of us who, aware of our curiosity or not, or afraid to ask, really do want to know who or what were Davis and Monthan.

JOHN L. KESSELL
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MICHAEL J. McDONALD and JOHN MULDOWNY. *TVA and the Dispossessed: The Resettlement of Population in the Norris Dam Area*. Knoxville: University of Tennessee Press. 1982. Pp. xv, 334. \$28.50.

As originally conceived, the Tennessee Valley Authority was more than a means of developing hydroelectric power, flood control, and river navigation. It encompassed a desire to modernize rural life through social and economic planning. In *TVA and the Dispossessed* Michael J. McDonald and John Muldowny analyze TVA's impact on the people whose homes and farms were lost forever. Using Norris Dam and Reservoir in eastern Tennessee as a case study, they focus on the initial phases of TVA operations—land purchase, family relocation, and grave removal. The construction of Norris Dam, TVA's first major project, required the purchase of one hundred fifty-three thousand acres of land and the relocation of over three thousand families. Instead of modernizing rural life, McDonald and Muldowny find, TVA left the dispossessed families worse off than before.

To set the stage, the authors describe the Norris Basin using demographic data, TVA surveys, and their own interviews with displaced families. Most of the region's inhabitants were subsistence farmers, the land was depleted and eroded, and overpopulation stretched resources to the limit. But TVA's priorities were always reservoirs and watersheds, never the needs of the indigenous population. The dispossessed families were left to relocate themselves in the same region, proving that nothing was done to relieve the overpopulation problem. Rising land prices and lack of good land meant that most displaced landowners could afford less land and poorer land than they had previously owned. Even the new TVA town of Norris never materialized as a model of what planning could accomplish. The delicate task of grave removal, however, went

smoothly since TVA worked closely with religious leaders. Despite TVA, the Norris Dam region still has one of the lowest standards of living in Tennessee.

McDonald and Muldowny attribute the failure of TVA as a modernizing agency to the well-known conflicts among the members of the board of directors. Arthur E. Morgan possessed a grand, even utopian, vision of TVA's mission, while his codirectors, David Lilienthal and Harcourt A. Morgan, held a more limited conception of their social role. Besides philosophical differences, each board member headed separate areas of operation. Lilienthal purchased the land, H. A. Morgan relocated the population, and A. E. Morgan handled grave removal. Thus TVA lacked administrative coherence.

McDonald and Muldowny's work is a valuable addition to TVA literature. This book is the kind of study that must be done about the New Deal—sharply focused at the local level, with analysis of programmatic results. But should programmatic analysis stress the short run or the long run? Should TVA be judged in terms of its ideal or practical goals? McDonald and Muldowny stress short-term effects, and they ignore long-term benefits such as cheap electric power. They also judge TVA programs in terms of ideal goals, which can seldom be realized. The challenge for historians, however, is to balance TVA's short-term losses against its long-term gains, and to understand the internal and external forces that shaped its programs.

DONALD HOLLEY
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JOHN D'EMILIO. *Sexual Politics, Sexual Communities: The Making of a Homosexual Minority in the United States, 1940–1970*. Chicago: University of Chicago Press. 1983. Pp. x, 257. \$20.00.

Previously, most histories of homosexuality have focused on Europe, except for some valuable articles and documentaries by Jonathan Katz. Now John D'Emilio, in this revision of his Columbia dissertation, has contributed an important study of the origins of the gay liberation movement. He uses conventional sources, and also the private papers of, and interviews with, lesbian and gay activists.

From research by Allan Berube, D'Emilio sees World War II as the crucial factor in the emergence of a homosexual community. As millions were taken from their families by the armed forces and defense industry, to largely sex-segregated, non-familial environments overseas or in large port cities, many homosexually oriented individuals found others like

themselves and began to develop a community. At the end of the war many of them remained in these cities, especially New York, Los Angeles, and San Francisco. The gay bar became the center of this developing urban subculture.

With McCarthyism, this emerging subculture was equated with communism as a danger to society, and employment was denied to thousands. Congress, the Eisenhower administration, the FBI, and the Post Office spent huge amounts of money investigating homosexuals and informing employers. Police directed brutal crackdowns on gay meeting places.

The aggressiveness of this discrimination promoted a backlash among gays, resulting in the 1951 founding of the Mattachine Society in Los Angeles. Founders wanted to mobilize a large constituency to combat discrimination and build self-pride. By 1953 Mattachine had two thousand members and began publishing a national gay rights magazine. But the leftist background of the leaders led to controversy, and more cautious leaders took over. The movement remained timid, small, and factionalized.

During the 1960s, however, both the sexual revolution and the civil rights movement had a profound impact on younger gay activists, who began picketing the government and the psychiatric establishment. San Francisco in particular became a center for gay protests. Black power and the counterculture, expressing pride in their differences, became models for radicalized young gays. In New York in 1969, after a police raid on the Stonewall bar, the arrested patrons began a riot that led to three days of street fighting between police and gays. The Gay Liberation Front was formed, and quickly spread to other cities.

Like the woman's movement, gay liberation continued in the 1970s, sharing ideology and the active involvement of lesbian feminists. Mass confrontation and public "coming out" of gay individuals broke the barrier of gays' fear of exposure, which set the stage for further gains.

D'Emilio presents a perceptive understanding of his subject, and writes about it clearly and well. But the book is almost too compactly written. More quotation from the activists themselves (especially Dorr Legg, Henry Hay, Del Martin, and Jim Kepner) would have made them come alive personally. Inclusion of photographs would also have aided readers in personalizing the people. Most surprising is the lack of a bibliography, though complete footnotes are included. The book is a treasure for gay activists, but it is also valuable as a case study in social change. Scholars cannot ignore the important impact of this minority movement in changing recent American social attitudes.

WALTER L. WILLIAMS
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MARY FRANCES BERRY and JOHN W. BLASSINGAME. *Long Memory: The Black Experience in America*. New York: Oxford University Press. 1982. Pp. xxi, 486. \$19.95.

American historians will be familiar with much of the information in *Long Memory*, but even specialists in black studies will be intrigued by the interpretations and synthesis offered by two of the most prominent of the younger black scholars who matured during the era of civil rights militancy.

Above all *Long Memory* is a detailed and generally accurate account of black resistance to racial oppression. There are occasional lapses into unfair misrepresentation, however. Research on the effects of ability grouping is inconclusive, but Mary Frances Berry and John W. Blassingame confidently inform their readers that "Black students suffered greatly from the 'tracking' system introduced in many school districts in the 1950s" (p. 282). The Coleman Report of 1966 indicated that there was substantial equality in measurable resources at majority-black and majority-white schools, but Berry and Blassingame state that "Once a school became more than 50 percent black, it was ignored by public officials" (p. 283). Their discussion of contraception and abortion is another example of one-sided exaggeration. Family planning clinics are disproportionately located in neighborhoods with heavy black population, the authors assert, and are part of a government-financed campaign "to eliminate black Americans" (p. 352).

Knowledge of American race relations has reached the point where it is possible to offer a balanced assessment of blacks and whites. In *Long Memory*, however, the focus is almost exclusively on white oppression and heroic black resistance, with little attention given to the tangle of social ills that have impeded black progress. The authors take pains to establish the persistence of African survivals and adaptations that helped blacks endure, but they generally ignore evidence that slavery and discrimination played havoc with the character of many blacks and the institutions of the black community. The "lower-class matriarchal family" is presented as "an example of the remarkable adaptation black institutions have made to oppressive conditions in America" (p. 84). The disproportionate incidence of crime among blacks is depicted as retaliation against an unjust socioeconomic system. The prison system is characterized as an "American Archipelago" (p. 257).

Unlike older surveys such as *From Slavery to Freedom* by John Hope Franklin and *From Plantation to Ghetto* by August Meier and Elliott Rudwick, *Long Memory* does not tell a story of change over time. Readers are left with the impression of unavailing black protest against persistent and undiminished

white exploitation. According to Berry and Blassingame, liberation cannot be achieved without "drastic measures" that are never precisely described (p. 220). At one point the authors maintain that criticisms of affirmative action have been unfounded, absurd, and even hysterical, but elsewhere they approvingly mention the recommendation of unnamed economists who call for "preferential treatment for blacks in business, apprenticeship, employment, and educational programs" (pp. 290, 220). Beyond this, Berry and Blassingame refer to "the monstrous forces of capitalism" and to the need for blacks to "rise up and break the chains of economic bondage" (pp. 195, 226). The American Communist party is depicted as a "consistent and often courageous" advocate of egalitarianism and as "the moral and intellectual inheritor of nineteenth-century abolitionists" (p. 223). More blacks would have joined the party, we are told, but they have been subjected to such a "massive barrage of propaganda" that "few of them knew about Russia's constitutional safeguards for minorities, the extent of equality of opportunity [in the USSR], or the equal provision of social services to its citizens" (p. 224).

RAYMOND WOLTERS
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ELIZABETH JACOWAY and DAVID R. COLBURN, editors.
Southern Businessmen and Desegregation. Baton Rouge:
Louisiana State University Press. 1982. Pp. x, 324.

This volume is a collection of essays discussing the role of businessmen in the early desegregation efforts of fourteen Southern cities. In the introduction, a valuable essay in itself, Elizabeth Jacoway argues that the portraits of Southern businessmen in Numan Bartley's *The Rise of Massive Resistance* (1969) "were too sharply drawn." She agrees that the businessmen were "conservative rather than reformist," but she believes that in the process of seeking social stability and economic progress they experienced a shift in values and accepted social change. This distinction between moderation for the sake of social control and moderation for the sake of ending white supremacy is critical to Jacoway, and she presses the point eloquently in her opening piece on Little Rock. Her fellow essayists, however, do not necessarily share her conviction.

Most of these essays deal with the first round of desegregation, the dramatic days of the late fifties and early sixties when black school children and college students challenged the racial barriers in public education and public accommodations. In some cases Southern businessmen learned from Little Rock and took the lead in organizing a vital center that sought to secure a delicate and peaceful

balance between the minimum amount of change the civil rights movement would accept and the maximum the white citizens would tolerate. Alton Hornsby, Jr., William Brophy, and Paul S. Lofton, Jr. in their respective studies of Atlanta, Dallas, and Columbia identify such a process that brought desegregation without violence. In other cases business leaders learned less from Little Rock, and either abdicated the center and did nothing, or, joined the right and resisted. New Orleans, Norfolk, and St. Augustine are such cases in point; Birmingham, Greensboro, and Memphis are more complex; and the records of Augusta, Jackson, Louisville, and Tampa are mixed.

James C. Cobb suggests that Augusta differed from Atlanta because it lacked a well-organized black community, and because it cared less about attracting new industries and projecting a New South image. Steven F. Lawson, in analyzing Tampa, expands on this observation, offering a New South-Old South model for explaining patterns of response to the civil rights movement. "Modernizing elites," he theorizes, "threw their considerable influence on the side of racial moderation" (p. 258). Lawson's model does not embrace Jacoway's optimism, however. Peace, prosperity, and a progressive image do not necessarily promise racial egalitarianism, and might just as likely be signs of a "strategy adopted by influential whites to maintain their social control over biracial community affairs" (p. 259). Similarly, William Chafe remains suspicious of Greenboro's "civility" and progressive rhetoric, and his essay reminds us that at every level of the movement women, blacks, and religious figures provided much of the moral and physical energy.

None of the authors in this volume is so cynical as to suggest that moderation was exclusively a ploy, or that necessary change did not take place under its aegis. But the half dozen authors here who look beyond the golden years of the movement to examine the post-1968 Thermidor are asking, implicitly at least, if "token desegregation" was not another critical moment in a halting revolution that began in 1954—or in 1865. And the more we reflect on the second Reconstruction—and the first—are we not the more likely to discover one continuous process, full of oscillation, suggesting that the way to understand this process as a whole is to compile case studies and to work toward a synthesis. This well-edited volume of original essays puts us a little closer to this aim.

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ARTHUR M. JOHNSON. *The Challenge of Change: The Sun Oil Company, 1945-1977*. Columbus: Ohio State University Press. 1983. Pp. xvii, 481. \$30.00.

During the 1950s and 1960s a number of histories of major American business firms were published, notably of oil companies. Most ended just before or after World War II, leading some of these firms in recent years to consider bringing their histories closer to the present. Sun Oil, which was among the last to do a history of the earlier years (August W. Giebelhaus, *Business and Government in the Oil Industry: A Case History of Sun Oil, 1876–1945* [1980]), has carried its history further in the present volume. Arthur M. Johnson, highly experienced in both oil and business history, was an admirable choice as author. He has made his focus the systematic study of the evolution of entrepreneurship in the Sun Oil Company during the postwar years.

At the end of World War II, Sun was a well-run, conservative, paternalistic, owner-managed, regional oil company, with J. Howard Pew, son of the founder, serving as its longtime president. In 1947 he was succeeded by Robert G. Dunlop, who had worked closely with Pew for a decade and who shared his deeply held laissez-faire philosophy. Pew remained a force to be reckoned with until his death in 1971. Decision making in those more simple and prosperous times tended to be reactive to perceived needs and opportunities rather than in accord with some well-integrated, long-range plan.

The year 1968 proved pivotal in Sun's postwar history. Because the company's management believed a company of Sun's size could no longer compete effectively in the increasingly costly search for oil, it sought a merger partner. Sun settled on the slightly smaller Sunray DX Oil Company, an efficiently operated integrated company in the Southwest and midcontinent, whose facilities dovetailed neatly with those of Sun. The merger caused Sun to enter into the first of three reorganizations, of which H. Robert Sharbaugh, successor to Dunlop as president in 1970, became the increasingly prominent principal architect. Believing that petroleum was a mature industry of limited future profitability (despite the windfall gains of 1974), Sharbaugh advocated a policy of "de"-integration, divestment, and diversification. Energetic and innovative, he led Sun down a decentralized managerial path similar to that pioneered by DuPont and General Motors a half-century earlier in which a small group of top executives carried on long-range planning and carefully monitored the results of the operations of a numerous body of subsidiaries. Sharbaugh's goal was that of turning Sun into a more profitable diversified management company.

His fatal error as a professional manager was in failing to reckon with the still substantial Pew family minority stock interest. Displeased by his emphasis on cash dividends, on which the family had to pay taxes, and displeased with his plans for heavy investments outside the oil industry, they forced him out

of the company in 1978 when the purchase of a large stock interest in a major medical supply house resulted in suits against Sun by the management of that firm and by the Securities and Exchange Commission.

Johnson has provided us with a volume that is illuminating throughout and is particularly fascinating for its discussion of the Sharbaugh era. His book should permit some interesting comparisons when Ben Wall's long-awaited postwar history of Exxon is published shortly.

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ROBERT H. FERRELL. *Harry S. Truman and the Modern American Presidency*. (Library of American Biography.) Boston: Little, Brown. 1983. Pp. viii, 220. \$13.50.

Robert H. Ferrell has been a leading contributor to the study of the Truman presidency. In this short volume, he sums up his research and thoughts on Harry S. Truman in a lively, forthright, but often unacceptable way.

Ferrell presents a sympathetic account of Truman's life, concentrating on his presidency. First we get the familiar farm-to-fame story of the Missourian's rise to power. The core of the book, five chapters on Truman's presidential years, follows. Reflecting the author's special interest, three of these chapters deal with foreign and military affairs. Ferrell telescopes most of Truman's domestic concerns into one chapter, with another treating only the issues of communism, corruption, the government's seizure of the steel mills, and the 1952 election. Consequently, most of Truman's second-term domestic policies get lost along the way. Ferrell concludes his book with four short chapters: one an account of Truman's retirement years; another that seems like an afterthought—a happy one—on the presidential institution under Truman; and two chapters on Truman's greatness as president.

One gets the impression that Ferrell wrote this volume hastily. Certainly, the book will be a critic's delight. Overstatements abound, such as that Winston Churchill was "the greatest statesman in British history" (p. 53) and "Herbert Hoover was the last of the old-time presidents" (p. 164). Then there are miscellaneous problems, for example, not explaining the 1940 Transportation Act after a long build-up to it; concluding unconvincingly about 1945–46 foreign policy that had Truman's "prescience been greater, had he spoken with the tongues of angels, he could have accomplished little more" (p. 62); and writing that developments in the Near East and

Latin America in 1947–48 were not connected to American concerns regarding Russia.

Ferrell's book is largely a defense of Truman, especially regarding foreign policy. He views the president's decisiveness as crucial in averting the possibility of a Third World War. Ferrell is not uncritical, though. He grants the president's overstatement in the 1947 Truman Doctrine address, and he questions Truman's recognition of Israel in 1948 (although the alternative of a United Nations trusteeship was hardly viable then). Ferrell criticizes the American drive into North Korea in the fall of 1950. Moreover, he asserts that Truman's failure to ask Congress to declare war against North Korea was a misjudgment.

Nevertheless, Ferrell has written essentially a case for Truman's greatness, citing his foreign policy, his institutionalization of the presidency, and his personal attributes, particularly his analytical abilities, decisiveness, and diligence. Ferrell's case for the defense is incomplete, however, for it substantially ignores Truman's domestic accomplishments. Moreover, the case for the prosecution will not rest, for much can be said in criticism of Truman. One hopes that less of the unfolding debate on Truman will focus on the secondary issue of whether he was great. After all, historians should primarily be concerned with delineating further and refining their assessments of the impact of Truman and his administration.

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FRED I. GREENSTEIN. *The Hidden-Hand Presidency: Eisenhower as Leader*. New York: Basic Books, 1982. Pp. x, 286. \$16.95.

JAMES C. DURAM. *A Moderate Among Extremists: Dwight D. Eisenhower and the School Desegregation Crisis*. Chicago: Nelson-Hall, 1981. Pp. xiv, 306. Cloth \$20.95, paper \$9.95.

In 1956 Samuel Lubell wrote in *The Revolt of the Moderates* that the "widely held image of Eisenhower as a five-star babe in the woods" was extremely misleading, and that indeed if one defined politics in its broadest sense—"the art of governing people through other people"—then there was little doubt but that Eisenhower was "a highly skilled professional, as complete a political angler as ever fished the White House" (p. 25). Few of Lubell's (or Eisenhower's) contemporaries shared such an assessment, however. Most agreed instead with Clinton Rossiter, who in *The American Presidency* (rev. ed., 1960) described Eisenhower as a man with "a dislike for aggressive politics and a horror of administrative detail," a president who, in Walter Lippman's telling

phrase, was never willing "to break the eggs that are needed for the omlette" (pp. 170–71).

Historians and political scientists are presently in the midst of a major reinterpretation of the Eisenhower presidency, an effort that has been very loosely labeled "Eisenhower revisionism." The phrase is perhaps unfortunate—both because it invites confusion with Cold War revisionism and because it lumps together rather a diverse group of scholars whose perspectives on Eisenhower often differ quite sharply. Like Lubell, most of these scholars tend to see Eisenhower as a fairly complex, skillful, and self-conscious political leader. It does not follow, however, that they therefore "like Ike," agree with his goals, or nostalgically yearn for a return to the supposedly placid years of the 1950s. What they do share is a willingness to see the Eisenhower presidency as a subject for serious historical investigation, to examine the rich archival collections that have been opened in recent years, and to challenge the accuracy of conventional interpretations. One of the two books under review in this essay makes an important contribution to this reassessment; the other does not.

In *The Hidden-Hand Presidency* political scientist Fred I. Greenstein argues that the key to Eisenhower's leadership, as well as its misassessment by contemporaries, was its covert character, that beneath the bland and amiable image he displayed in public was a skilled, if secretive, political operator, and that behind his "seeming transcendence of politics was a vast amount of indirect, carefully concealed effort to exercise influence" (p. viii). Eisenhower's covert exercise of power, Greenstein continues, was based on six interrelated and overlapping "strategies"—"hidden-hand" leadership, or the camouflaging of his own "political" activities; the "instrumental" use of language, which is to say that he was at times deliberately evasive, ambiguous, and even untruthful; the refusal to "engage in personalities" in public, even when, as in the case of Senator Joseph McCarthy, he was widely criticized for so doing; the shrewd assessment of both friends and foes and the careful calculation of the help or harm that they might do him in any given situation; the selective delegation of authority to subordinates, allowing them considerable leeway over tactical details, while at the same time using them as lightning rods to deflect criticism from himself; and, finally, the artful management of his own public image. It was no accident that Eisenhower was so popular, Greenstein concludes, or that he was the last of our modern presidents to successfully complete a second term.

Eisenhower was not a political genius or "Machiavelli in pinstripes," however. As Greenstein is careful to note, Eisenhower's exercise of "hidden-hand leadership" did not invariably lead to success. Greenstein cites Eisenhower's failure to dissuade

Richard Nixon from running for vice-president in 1956, but a careful survey of the recent literature would produce many other examples as well. More important, Greenstein suggests that much of the Eisenhower style is not transferrable, that its success depended on a particular political climate, on the particular skills that Eisenhower brought to the office (to say nothing of his stature as a war hero), and on the relatively modest and incrementalist goals he held. To which we might add that the requirements of leadership, and hence the criteria of success, during a period of imperial ascendancy and growth such as the 1950s, are quite different from those appropriate to an era of decline and stagnation such as the present. All of which should come as cold comfort to those who have been seeking to "Eisenhowerize" the present occupant of 1600 Pennsylvania Avenue.

There are limits to this thoughtful study, most of which Greenstein himself recognizes. There is virtually nothing on Eisenhower as a party leader, for example, even though such a study might have nicely illustrated the limitations of the "hidden-hand." Nor does Greenstein deal with the goals of Eisenhower's leadership. "My analysis," he reminds us, "has been of his *mode* of leadership, not of the merits of his policy aims" (p. 228). What seems remarkable to me, of course, is the high degree of congruence, if not identity, between Eisenhower's goals and his style. Nor, finally, is Greenstein correct in labeling Eisenhower's domestic views "laissez-faire," a characterization that does considerable violence to the complexity of his views on political economy. Greenstein's presentation is highly schematic, moreover, and his evidence more illustrative than conclusive. This is, nevertheless, a very good account of Eisenhower's leadership and an important contribution to the study of the American presidency.

By contrast, James Duram's study of Eisenhower and the school desegregation crisis makes almost no significant contribution to our understanding of this important topic. Indeed it is a book flawed by inadequate research, faulty analysis, inept organization, and poor writing. Like Greenstein, Duram has explored the recently opened collections of the Eisenhower Library. He has failed to supplement this rich collection with any of the many other important archival sources bearing on the topic, however, including the papers of the NAACP, Americans for Democratic Action, or Lyndon Johnson. Nor, more importantly, has he utilized the existing secondary literature. Unaccountably missing from his bibliography and footnotes are many of the most important works on civil rights in the 1950s, among them Steven Lawson's excellent study of black voting rights, Numan Bartley's classic account of massive resistance, Foster Rhea Dulles's

history of the Civil Rights Commission, J. W. Anderson's study of the 1957 civil rights act, and both Daniel Berman's book on the 1960 law and his subsequent study of the Supreme Court and desegregation. Nor, for that matter, does Duram cite E. Frederick Morrow's *Black Man in the White House*, a memoir by the only prominent black on Eisenhower's White House staff.

Unhappily, this is not all that is missing. In what should be one of the books most important chapters—"The Shaping of a Moderate Mind"—the author manages to avoid any discussion of Eisenhower's views on race. Indeed nowhere in the manuscript does he confront this critical issue, ignoring entirely, for example, Arthur Larson's account of Eisenhower's opposition to "social equality" and to the notion, as Eisenhower phrased it, "that a Negro should court my daughter" (*The President Nobody Knew* [1968] pp. 124–33). Such an omission, in a book purporting to deal with Eisenhower's role in implementing (or, as it turned out, not implementing) the *Brown* decision, is truly difficult to comprehend.

When the history of Eisenhower's response to the black struggle for equality is written (and as this review should make abundantly clear, this very much remains to be done), it will reveal the limits of a decent but prejudiced man who greatly preferred public order and tranquility to the turmoil of democratic social change. It will reveal as well the often narrow limits of the "hidden-hand presidency."

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ROBERT A. CARO. *The Years of Lyndon Johnson*. Volume 1, *The Path to Power*. New York: Alfred A. Knopf. 1982. Pp. xxiii, 882. \$19.95.

A biography, like any other study, must organize its material around a thesis, and the thesis necessarily narrows even as it gives form and clarity. But a biography is about a contradictory, changeable human being, whose interior life is a continuing flux of thought and emotion that his behavior and social personality shape into some degree of orderliness. Robert A. Caro's study of Lyndon Johnson chooses a clearly judgmental thesis and presses the whole of his subject within it.

Caro nicely sets his argument in the first sentences of his work: "Two of the men . . . that day in 1940 were rich. The third was poor." He tells of a Lyndon Johnson brought up in genteel poverty and driven thereafter by ambition for money and political power. Caro writes beautifully: of the Texas hill country, for example, where Johnson was born and

raised, a land once so lush that it had tempted its first white inhabitants to expect wealth from it, while cattle, drought, and prairie fires periodically turned its green grass to brush that then gave way once more for a time to the seductive green. Hill country people could be wracked by poverty while the land around them, in its moments of fevered brilliant green, tortured them with dreams of prosperity. Young Lyndon, son of a state politician ravenous for success but too honorable or romantic to purchase it, scraped and planned. In concise and cleanly narrative anecdotal prose, Caro goes on to tell of the schemes and hungers with which Johnson built a political and then a presidential career. The story does not tire, and Caro makes of it not merely the study of an individual but implicitly an inquiry into the ruthless "drivenness" and the finagling that are one side of American politics. The sources Caro employs are at times themselves one-sided or at least it seems that he relies more on sources we know to be hostile toward Johnson. This was often the generic approach of Long Island's *Newsday*, which Caro wrote for in its great days.

Caro has strong moral antagonisms toward Johnson for his political and financial methods and for the Vietnam War, and as a journalist Caro writes in the tradition of the muckrakers, interested less in the mind and outlook of a politician than in the schemes and lies of political life. What he does not discuss at sympathetic and inquisitive length is the ideological and political composition of the young politician who later as president would champion the cause of full racial equality, social and economic as well as legal, while black militants insulted him and whites, who could have been as politically useful to him as the black community, turned hostile. Caro does not explain, at least in this volume, what precisely might one day lead Johnson further and further into a disastrous war that was of no political use to him, in defense of a government that certainly looks in retrospect no less brutal or repressive than its Communist successor. Were these, too, expressive of a man so combative and so committed to imposing shape on the world around him that he could at times forget his more mundane ambitions?

But then something other than sheer disembodied will to power and wealth must have animated Johnson. Ideology, moral dedication, a certain understanding of what the good society might be and of what the nation's diplomatic obligations abroad required, and stubborn and wrong-headed loyalty to Saigon may have informed his will to power and told him what achievements his pride would pursue. If so, we have in Johnson a more complex moral phenomenon than muckraking alone, even of the subtle and stylistically superior sort that Caro can command, will be able to explain. Suppose that Johnson's blatant and unquestionable hunger for

money, and his unquestionably assertive ego, were manifestations of a personality so appetitive that it needed to conquer some large portion of the world around him. And suppose that Johnson was sufficiently conventional in his morality to assume that the relief of poverty in this country, faithfulness to our allies abroad, and the defense of a regime that was at least weak enough to permit the existence of some opposition forces were moral objects large enough to vindicate the honor of the president who pursued them? Then we would be stuck with that mixture of virtue and self-concern that is perhaps the legitimate and certainly the inescapable human lot. Muckraking does not address this kind of question.

Johnson's presidency was in its own way singularly moral, forever pursuing—with what a more timid politician would have considered a wrong-headed consistency—schemes for conquering poverty or improving education or preserving a scrap or a hope of freedom in South Vietnam. Whether the Lyndon Johnson who in his later years had become the constant liberal Democrat was necessarily good for that reason, as the wealth and office seeker was unsavory, is not necessarily clear. But *that* Johnson seems to have existed, and knowledge of him, even of the traits of impatience and assertiveness that attended his later politics, needs examination. Johnson was at least one of the most morally interesting presidents in American history. There ought to be a study of this Johnson. Perhaps Caro's later volumes will provide it. At any rate, a journalist and historian of uncommon persistence, literary grace, and power to order and compose is at work.

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MICHAEL TURNER. *The Vice President as Policy Maker: Rockefeller in the Ford White House.* (Contributions in Political Science, number 78.) Westport, Conn.: Greenwood Press. 1982. Pp. xvii, 252. \$29.95.

The modern vice-president is ordinarily viewed as a president-in-waiting, positioned either to assume the Oval Office on the president's death or to inherit his mantle after his term. Nelson Rockefeller's case was different; well beyond the age for apprenticeship, he brought administrative experience and political prestige to a uniquely vulnerable administration. Yet his hopes to contribute dynamism and vision to the Ford presidency were largely dashed. As Michael Turner demonstrates, Rockefeller's vice-presidential years present "a study in adversity" (p. 152). Drawing on interviews with Rockefeller and his associates, as well as vice-presidential papers that

Rockefeller made available to him, Turner has written a careful and intelligent study whose documentation is unusually rich for such recent events.

Nelson Rockefeller's two major ventures in the Ford administration were the attempt to shape domestic policy formulation through domination of the Domestic Council and the effort to commit the administration to a new government corporation that would finance massive investments in energy production. When Rockefeller was asked by Ford to accept the vice-presidency, he was offered a mandate to direct domestic policy in a fashion analogous to Henry Kissinger's reign in foreign policy. The lengthy delay in his congressional confirmation, however, brought Rockefeller into a domestic policy realm where others had already staked out their turfs. As Turner shows in detail, Rockefeller found himself in an impossible situation, opposed on one side by a suspicious White House staff headed by Donald Rumsfeld and on the other by cabinet departments who had been granted considerable independence and influence. Finally abandoning the Domestic Council as his policy vehicle, Rockefeller moved to a "social programs review" designed to lend a creative tone to Ford's election-year legislative program. His work here fell afoul of the president's budget cutting: "when Ford announced his State of the Union proposals on January 19, 1976, virtually all of Nelson Rockefeller's recommendations were conspicuous—by their absence" (p. 86).

Rockefeller did somewhat better with his plan for a federal energy development corporation—yet the ultimate result was equally disappointing. Seizing upon President Ford's call for greater energy self-sufficiency, Rockefeller promoted a government corporation that would finance energy projects too large or risky for private companies to undertake on their own. Although Ford was attracted to the plan, it drew vocal opposition from his conservative economic advisers and from big business. Even though the president ultimately proposed an energy independence authority, his administration remained so divided on the subject that the legislation died quietly on Capitol Hill.

The frustrations that Rockefeller experienced reflect the inherent institutional weakness of a vice-president who sought to be more than a generalist and occasional policy adviser. But they also reflect the clash between a vice-president with drive and large plans and a president more given to caution and less certain about the policy direction he wished to pursue. Turner's sympathies are with Rockefeller; the vice-president's problems, he suggests, "sprang in large measure from the ambivalence, indecision, and imprecision that characterized Ford's leadership style" (p. 179). Nelson Rockefeller overestimated the influence he could wield with such a president. *The Vice President as Policy Maker*

provides a valuable account of just how formidable the obstacles to his influence proved to be.

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CANADA

NOËL BAILLARGEON. *Le Séminaire de Québec de 1760 à 1800*. (Les Cahiers d'Histoire de l'Université Laval, number 25.) Québec: Les Presses de l'Université Laval. 1981. Pp. 297. \$20.00.

At a time when many microhistories are published with titles that promise disclosure of great truths, it is refreshing to read a book that lives up to its modest title and does a little more besides. Noël Baillargeon started off with an advantage. Since its foundation in 1663, the Quebec Seminary preserved most of its own records as well as acquiring a mass of public and personal documents. All of these riches are kept in a well-run and superbly catalogued private archive. In short, the seminary was a natural candidate for an institutional history, and Baillargeon, in two earlier volumes, has told its story up to 1759, when the capital of New France was surrendered to British forces.

Like all Roman Catholic institutions in Canada, the Quebec Seminary had to accommodate itself to foreign rule under a Protestant monarch. In addition to this awkward situation, the seminary had lost property in the bombardment of the besieged city and through General Wolfe's policy of crushing resistance among the countryfolk with fire and sword. The revenues from what remained of the rural estates were insufficient to pay for the reconstruction of the seminary. "The destruction of its properties represented . . . a loss of some twenty-three thousand *livres* in earnings. At the end of hostilities, the seminary could count only on its properties in France and the seigneurie of Île-Jésus, apart from the mills of Baie Saint-Paul, in order to survive" (p. 13). Moreover, the income from France was endangered by the British government's desire to sever all links between the church in Canada and the French church. Although the mother house in Paris advised caution until the fate of the colony was determined by peace treaty, the directors at Quebec plunged ahead with reconstruction.

What is remarkable is that, in these most trying circumstances, the Quebec Seminary survived while assuming a new and "unexpected mandate": secondary education for males with or without a religious vocation. Before the conquest the seminary had consisted of the *Petit Séminaire*, a grammar school for young boys, and what was called *le Grand*

Séminaire. Despite its name, the "Large Seminary" was no more than a residence for a handful of priests and for clerical novices attending the nearby Jesuit college. The subsequent closure of the Jesuit college forced the seminary in 1765 to open what became a classical college. This was done to ensure that Canadians would continue to receive local training for the priesthood; the fact that the college provided young men of secular ambition with a higher education was an incidental benefit. Yet it was the Quebec Seminary that was the nucleus for French Canada's first university, *l'Université Laval*, in 1852.

As late as 1793 the superior hesitated to call the school a classical college. The courses offered were very limited, and the instruction was, by French standards, out-of-date. Lecture notes—often taken verbatim from books—were dictated to the students in Latin. The basic curriculum consisted of theology, logic, rhetoric, and philosophy. At the end of the eighteenth century mathematics, physics, and geography (memorized in rhyming couplets) were inserted into this traditional curriculum. According to Baillargeon, this program and the austere discipline of the seminary discouraged Canadian aspirants to the priesthood. The spirit of seventeenth-century Catholic rigorism was maintained within the church. In 1795 Bishop Jean-François Hubert echoed the sentiments of preconquest administrators when he described Canadian males as "unaccustomed to the yoke and to restraint" (p. 110). The relatively small number of clerical vocations might also have been due to the worldly climate of the late eighteenth century.

Canadians chafed under clerical authority before 1760, and this restiveness increased under British rule. The Catholic laity seemed ready to exploit the perceived weakness of the church; this is apparent in the relationship of the seminary with its tenant farmers, mill lessees, and residents of parishes of which the seminary was the patron. A social gulf as well as a difference in material interests divided the seminary's directors from these dependents. The latter connived endlessly to reduce their obligations and enlarge the benefits provided by their clerical lords. The residents of one parish induced the seminary, as patron, to contribute generously to the rebuilding of their church, and then they loudly complained that the seigneurial pew interfered with their view of the altar. This episode, according to Baillargeon, was "typical of the mentality and customs of the age" (p. 233). He does not explain this comment and is reticent about making any trenchant judgments. There are, however, quotations from documents that reveal the human failings of bishops, priests, and laymen.

Le Séminaire de Québec de 1760 à 1800 is a sympathetic account that relies primarily, but not exclu-

sively, on the seminary's archive. Yet the book is not a filiopietistic chronicle of this one religious house; the author is too good a historian to slip into that mode. This is a clearly written and, on the whole, even-handed narrative history. The directors, as landlords, may have been more self-interested in their defense of the seigneurial system than the author admits. Since the bishop of Quebec was the senior member of the seminary, the book deals with the internal politics of his huge diocese as well as the struggle of the church to retain its institutional integrity. The bishop and the seminary, by producing more Roman Catholic priests, frustrated the British plan of gradually incorporating the church in Canada into the Anglican communion.

In tracing the evolution of an impoverished branch of a French seminary into a prosperous diocesan college staffed by Canadians, Baillargeon describes the destruction of war, the ensuing decade of reconstruction, the prudent management of financial resources, and the expansion of the academic program. To expand its educational services in lean times, the seminary withdrew from work in parishes and missions, including Cahokia, Illinois. Without acknowledging the fact, this book fits into the principal theme for the history of French Canada after 1760: survival in adversity.

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GREGORY S. KEALEY and BRYAN D. PALMER. *Dreaming of What Might Be: The Knights of Labor in Ontario, 1880–1900*. New York: Cambridge University Press. 1982. Pp. xv, 487. \$49.50.

The Knights of Labor were a powerful factor in Ontario in the late nineteenth century, yet no modern history of the Knights has been written. Gregory S. Kealey and Bryan D. Palmer, two of the most gifted Canadian practitioners of the new labor history, have attempted to fill that gap, with mixed results. *Dreaming of What Might Be* is a detailed, carefully researched, and ambitious study; unfortunately, it also is curiously organized, difficult to read, and hyperbolic in its claims for the collective hero, the Knights of Labor.

If one did not know better, one might suspect that this was a revamped doctoral thesis. The characteristics are here, including an argumentative review of the literature, lumpy clusters of statistics, and dissertation-like reluctance to trim unnecessary detail. The writing is often well above thesis standard but is very uneven, and the meaning sometimes becomes obscured in a blizzard of poorly defined, or undefined, jargon. An example is the chapter on working-class culture. Although the body of this chapter is lively and effectively written, it begins by putting

the reader through an endurance test, which requires him to push through such verbiage as "movement culture," "totalizing," "alternative hegemony," "residual class culture," "associative and solidaristic principle," to find that "the emergent culture . . . is best understood in terms of preemergence" (pp. 278–80).

The reader who does not give up the struggle across this snowy landscape will find much of value. Despite its unpromising beginning, the chapter on working-class culture is a clever analysis of the ritual, rhetoric, and leisure activities of the Knights. If it does not convince that "an alternative hegemony" seriously challenged the established hegemony in the 1880s, it does make clear that important changes in working-class culture were in train. These changes helped workers respond to the advent of industrial capitalism. The sections on the Knights in politics tend to be overly enthusiastic about the small victories achieved, but they provide a necessary antidote to the bleakly negative analyses of earlier writers, such as Martin Robin and Bernard Ostry.

Despite its great detail, the book is not a rounded history of the Knights in Ontario. The focus is sharply on Toronto and Hamilton, the areas we already know most about thanks to earlier books by these authors. After a statistics-littered overview of the "structure" of the Knights, they rush on to close studies of Toronto and Hamilton. Not only do these accounts lack originality, but they also might bewilder the reader unfamiliar with the general context in which the experience of the two cities must be situated. That context is provided only after the Toronto and Hamilton chapters, and then far from completely. This organization not only creates confusion but also leads to a good deal of repetition. Despite all this, the Hamilton chapter is lively and offers the book's most interesting analysis in its treatment of the Knights' appealing and romantic "modern chivalry."

There is much more to recommend in *Dreaming*. Nevertheless, both the past accomplishments of the authors and the vastly inflated price of the volume lead one to expect better writing and organization, less historical cheerleading, and tougher questioning than are delivered here.

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LATIN AMERICA

NICHOLAS P. CUSHNER. *Farm and Factory: The Jesuits and the Development of Agrarian Capitalism in Colonial Quito, 1600–1767*. Albany: State University of New York Press. 1982. Pp. ix, 231. Cloth \$42.50, paper \$13.95.

Nicholas P. Cushner has given us a second solidly crafted and useful work on Jesuit agricultural enterprise in colonial Spanish America (with a third volume, on colonial Argentina, scheduled to appear during 1983). As the title of the book makes clear, Cushner places himself firmly in the camp of those scholars who view large-scale colonial Latin American agricultural enterprises as profit-making concerns, and therefore capitalist in nature, rather than prestige-oriented and feudal in the Old World mold. In a workmanlike and far-ranging introduction and an even better conclusion, he addresses himself to these issues within a comparative framework. But the real strength of the book rests on its careful, empirical reconstruction of Jesuit farming and textile-manufacturing enterprises in the area of Chillos, near the city of Quito, in what is today Ecuador. The work is based on exhaustive research in Jesuit archives, primarily in Rome and Quito, supplemented by documentation from local non-church sources. We are given chapters on the impact of the Spanish conquest on the social, economic, and political structure of the Quito area; on the distribution of land in the region and Jesuit acquisitions; on the economics of farming and *obraje* textile production; on rural labor; and on Jesuit finance, capital formation, and accounting practices. Among the most interesting parts of the book are those on the organization and technology of textile production and on Jesuit accounting techniques compared to those of contemporary Europe. Some of Cushner's interesting conclusions are that low wages to Indian labor constituted a kind of subsidy to Jesuit enterprises, which helped to account for their relatively high profitability; that large, rural estates were not a direct outgrowth of the process of conquest; and that Jesuit enterprise was not particularly advanced technically or economically compared to lay-owned properties.

In light of the book's considerable strengths, it is disappointing to see the author skate over some interesting or difficult theoretical points and their implications. For example, though the question of Indian labor is central to his work—in his definition of a labor force in relation to the putative characteristics of agrarian capitalism, and in his discussion of the economics of textile and agricultural production—he dismisses the local demographic situation in a few sentences at most. In lieu of a convincing demographic (or other) explanation for the consistently low wage-structure of rural and *obraje* labor, Cushner concludes (p. 135) that wages remained low because the Spanish considered Indians inferior. If this line of reasoning were true, then we should expect to see European antiblack attitudes keep New World slave prices low, too, but we do not. The author leads us into several other such theoretical *cul de sacs*, as well, a notable one being his

interesting but ultimately truncated speculations on the differences between Jesuit and lay management practices. Because of this tendency, the book is finally more descriptive than analytical; its major fault is a shortage of questions and a surplus of information. Nonetheless, this is a valuable contribution to our knowledge not only of Jesuit enterprise in particular but also of colonial economic history in general.

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IAN JACOBS. *Ranchero Revolt: The Mexican Revolution in Guerrero*. (The Texas Pan American Series.) Austin: University of Texas Press. 1982. Pp. xxii, 234. \$25.00.

Here is a book for Mexican historians to hand to their colleagues who study the United States or Europe. It represents the best in recent historiography while telling an entertaining and instructive story of people caught in the pressures of modernization and revolution.

Seemingly, Ian Jacobs has chosen a corner of Mexico unswept by the broom of the Porfirian dictatorship (1876–1911) or of the revolution (1911–40), but we learn that both left significant marks in Guerrero. He investigates three broad themes of modern Mexican history in the confines of this southern state: politics and economy during the dictatorship (land tenure patterns only in the northern section because of limited documentation), the revolution and the rise of the revolutionary state, and agrarian reform (again limited to northern districts). His examination of political developments during the Porfirian years demonstrates that the struggle between the regional political boss (for most of the nineteenth century Juan Álvarez) and the national government led to the dictator's intervention and generated hostilities that boiled to the surface in a revolt in 1901 and provided the major grievance for those joining the revolution of 1911. He finds that the dictator's economic program led to little modernization and that here (as in sections of the Bajío) the Liberal disentailment policy actually worked, creating smallholders (rancheros). This pattern differed substantially from the rapacious haciendas expanding onto village-owned properties in neighboring Morelos (home of Zapata). Haciendas existed in Guerrero, but owners rented lands to villagers. The revolution in Guerrero, then, was a movement of middle-class rancheros, school teachers, small merchants, and professionals. They defined their goals in political terms: they demanded free elections and local autonomy. Jacobs follows the shifting patterns of local alliances and intrastate

rivalries that reflected no ideological concern but rather the traditional geographical splits in the state.

Not until 1919 did Guerrero have an agrarian problem. Then, as an instrument of national-political invention, an agrarian reform program was launched to provide lands to villages; the twin result was to create antagonisms throughout the society and to create a crowd of agrarians dependent on the national government. By 1940 Guerrero had been returned to a position subordinate to Mexico City, but, contrary to the personalistic ties that bound it to the dictator, the revolution had established a bureaucratic system that incorporated many of the old local families and had created a popular base of support through its agrarian policy.

This is an excellent monograph that builds on the local and regional studies initiated by Luis González and Michael Meyer and contributes to the analysis of social and economic backgrounds of the revolutionaries done most successfully by Ramón Ruiz. Here is an author who is truant from the schools of history, refusing to attend social or quantitative or Marxist or traditional history. He has adopted the methodology demanded by his sources. The only weakness in the study is that some of the protagonists, such as the Figueroa brothers, appear as pasteboard figures. Undoubtedly, this is because Jacobs was denied access to the extensive Figueroa family archive.

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MICHAEL MONTEÓN. *Chile in the Nitrate Era: The Evolution of Economic Dependence, 1880–1930*. Madison: University of Wisconsin Press. 1982. Pp. xviii, 256.

THOMAS F. O'BRIEN. *The Nitrate Industry and Chile's Crucial Transition, 1870–1891*. New York: New York University Press. 1982. Pp. xv, 211. \$30.00.

From the War of the Pacific (1879–83) to the Great Depression, nitrates were "the wage of Chile." Nitrates financed expansion of the Chilean state, fueled the growth of commerce, banking, and agriculture and spawned a militant labor movement that became the vanguard of the national working class. The nitrate era also witnessed the surrender of Chile's most valuable economic resource to foreign control, cementing the pattern of neocolonialism that has characterized Chile's economic development ever since.

It is the common emphasis on Chile's "dependency" vis-à-vis the developed capitalist world that leads the authors of these two books to ask some of the same questions and often to find similar answers. Both authors first critique dependency the-

ory and then cautiously embrace it. They agree that dependency, which antedated the War of the Pacific, was consolidated by the Chilean decision to permit foreign ownership of nitrate deposits in the annexed provinces and the quick establishment of British dominance. This arrangement served the Chilean elites very well, sparing them the dangers of rapid socioeconomic change that might have resulted from direct national exploitation of nitrates, while providing abundant tax revenues for the state and those who controlled it. Politics in the nitrate era was essentially an intracelite struggle over the distribution of nitrate revenues, and the 1891 civil war was the escalation of these rivalries during a period of crisis in the nitrate economy.

Beyond this common ground, the books are apple and orange. Michael Monteón's study rests on the thesis that "Chile's modern history consists of the relation of her export-based economy to the political organization of power" (p. xii). He examines that relationship in a chapter of background, five chapters on the 1880–1930 period, and a conclusion that traces the course and consequences of dependency to the present. The core chapters emphasize the workings of the nitrate economy, the 1891 civil war, politics in the "Parliamentary Republic" (1891–1924), labor and the early left, the rise of United States influence in the 1920s, and the policies of President Carlos Ibáñez (1927–31). The chapter on labor and the early left, based on extensive research in government reports and labor newspapers, is especially informative. Although he offers new data and fresh insights into important aspects of Chile's economic and political development, for the most part Monteón is treading a well-worn path through this period of Chilean history. The principal contribution of his book is not the amount of new material he uncovers but the application of a provocative interpretation to the general history of the nitrate era.

Despite its merits, Monteón's book should be used with caution. The analysis depends heavily on United States and British diplomatic and consular reports, which, given the cultural biases and linguistic limitations common among their authors, should not be relied on for substantive political and economic information that is readily obtainable from Chilean sources. Important works of central relevance to Monteón's themes, including books and articles by Julio Heise González, Arturo Valenzuela, Brian Loveman, James Morris, Ricardo Coughmondjian, Paul Drake, and others, are conspicuous by their absence in notes and bibliography. These problems, combined with occasional lapses of argument (for example, President Alessandri's response to labor demands, pp. 142–43), errors of fact (the year congress suspended the tariff on livestock, p. 102), questionable interpretations (the implica-

tion that the Radical party in 1918 was "not of the elite," p. 131), and recurring misspellings of Spanish terms, make the book uneven in quality and less authoritative than one would desire.

Thomas F. O'Brien's work focuses on "the interaction between a traditional society and the capitalist center" (p. xii), and, although deliberately eschewing dependency terminology, he analyzes in detail the causes and effects of foreign domination of the nitrate industry. His focus is narrow: the two decades between the beginnings of nitrate mining in Tarapacá, then a province of Peru, and the 1891 civil war. His first two chapters examine nitrate activity in Peruvian Tarapacá; chapter 3 analyzes the Chilean wartime decision to recognize British claims to property in the occupied territory; chapters 4 through 7 cover the 1880s, focusing on the emergence of the foreign nitrate monopoly and its accommodation to Chilean society and politics; and chapter 8 treats the 1891 civil war. O'Brien demonstrates that Chilean nitrate entrepreneurs, products of a traditional society with limited credit, managerial, and technological resources, soon faltered in the intense competition with Europeans in Peruvian Tarapacá, leaving the province firmly under foreign control when it was annexed by Chile. The "crucial transition" from foreign dominance of international trade before 1880 to direct control of the primary economic resource after that time, while enriching the elites, propelled Chile into a state of "dynamic stagnation" that continues today.

O'Brien's book is well grounded in Chilean, British, and Peruvian sources. It is tightly argued and smoothly written. Its sections on nitrate operations in Peruvian Tarapacá and on the symbiotic relationship between British nitrate producers and Chilean society are especially incisive. The interesting collective biography of Chilean nitrate entrepreneurs, as well as other parts of the analysis dealing with motives and attitudes, would have benefited from the insights that contemporary memoirs and novels offer into the mentalities of the protagonists.

Together, these new studies substantially advance our understanding of an important period and a crucial set of developments in Chilean history. Monteón's work combines generous detail with an interpretive analysis of the entire period. O'Brien's book not only sheds valuable new light on the early nitrate era in Chile but succeeds also in its objective of providing specific historical information useful in "clarifying the dependency hypothesis" (p. xiii).

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GERHARD JACOB-WENDLER. *Deutsche Elektroindustrie in Lateinamerika: Siemens und AEG, 1890–1914.* (Bei-

träge zur Wirtschaftsgeschichte, number 16.) Stuttgart: Klett-Cotta, 1982. Pp. 376.

Focusing on Latin America, Gerhard Jacob-Wendler ameliorates the scarcity of thorough historical market analysis relating to the Second German empire's explosive industrial expansion, which dictated the constant necessity to develop new foreign markets. While European neighbor states and the United States were becoming increasingly protectionist, Latin America beckoned as a surviving free trade area. The emergence of the exquisitely modern electrical industry, probably the most viable manufacturing sector of the German economy, was thus destined to affect Latin America, especially prospering Argentina, as well as Brazil, Chile, Mexico, and Uruguay. Two dominant corporations, AEG (Allgemeine Elektrizitätsgesellschaft) and Siemens, bested repeatedly but not inevitably their strongest, mostly American and British, competition.

The larger electrical companies replaced traditional commercial middlemen with more widespread and more economical direct company outlets in Latin America, which provided essential spare parts more reliably and offered more adequate merchandise inventories. The Germans reinforced their marketing position by providing generous credits to clients in capital-starved Latin America with funds furnished by large German banks. Indeed, by 1914 the AEG industrial-financial empire had become the largest German export enterprise and not only owned electric utilities and streetcars in Argentina, Chile, and Uruguay but also contributed significantly to the global eminence of the German electrical industry in general.

German competitiveness, especially against Americans, was enhanced by attentiveness to merchandise preferences of Latin American clients and sensitivity to Latin American customs in general and business practices in particular. The Germans reaped additional benefit from saturating promising provincial markets and from a merited reputation of maintaining reasonably consistent pricing policies that remained insulated from personal seller-client relationships. Reflecting some devious predilections nonetheless, German entrepreneurs sometimes attracted the business of nationalistic British clients by camouflaging German business firms, especially in Chile, as British enterprises.

There existed German inefficiencies, however. Latin American operations suffered on occasion under inexperienced sales and production managers, from a sometimes rapid managerial turnover, and from an overblown autonomy of a number of executives from supervision by company headquarters. German electrical corporations were also vulnerable to cost and inventory control problems and

to employee complacency as well as low profit margins. In this context, it is noteworthy that AEG and Siemens were strenuous competitors for shares of the Latin American markets.

Jacob-Wendler's painstaking research into company archives, newspapers, technical journals, and specialized secondary sources provides unique insights into an important phenomenon of modern times. Yet conceptualization and style are flawed by poorly integrated and lifeless narrative histories of businesses relating to Germany and key Latin American countries. Included, unfortunately, are one-sentence "paragraphs" and excluded, regrettably, is a much-needed index.

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MARK FALCOFF and FREDERICK B. PIKE, editors. *The Spanish Civil War, 1936-39: American Hemispheric Perspectives*. Lincoln: University of Nebraska Press, 1982. Pp. xxiv, 357. \$21.95.

These essays analyze reactions to the Spanish Civil War in Mexico, Cuba, Colombia, Peru, Chile, and Argentina. The preface by Mark Falcoff compares the "operative myths" concerning economic and political development in Europe and the United States with those in the Spanish-speaking world. Before the Civil War era, he says, Spanish Americans clung to their cultural heritage despite copying French intellectual trends and surrendering to Anglo-Saxon economic strength. But the fall of the monarchy "shifted the cultural ground on which so many Spanish Americans had long assumed they stood" and "the Mother Country herself was now seen shopping for ideologies and agendas in the familiar marketplaces of Northern Europe and even the Soviet Union." Franco's appeal was to traditional elites searching for alternatives to "urbanization, secularization, the entry into the political scene of an organized and militant proletariat, [and] the creation of an adversary intelligentsia," while revolutionary elites turned to the populist ideals of the Spanish Republicans.

Within this framework, Spanish American countries responded to the Iberian crisis according to national attitudes and circumstances, particularly the status of *peninsulares* in the population, the stance of the church and the military, and the stage of national development. For instance, T. G. Powell describes Mexico as "hispanophobe" as well as "anti-fascist" because of racial complexion, anticlerical policies, revolutionized armed forces, and radical politics. He also scorns Mexico's claim to be the heartland of modernization in Hispanic nations and to have held out for a more democratic Spain until *El Caudillo's* death. Powell seems to agree with

Mexico's conservatives that only by abandoning Cárdenas's revolutionary dictatorship has Mexico itself managed a real start on modernization.

Some Spanish American nations looked to the Spanish Republic as a middle way between the Old Regime and the modern materialist ideologies of the United States and the Soviet Union. The "Black Legend" of a backward Spain was replaced by the "White Legend" of a brutalized people bringing suppressed democratic urges to fruition in the republic. Alistair Hennessey describes Grau and Batista rivaling each other in voicing support for the moderate Republicans because 80 percent of Cubans, including most *peninsulares*, favored the Loyalists, for example. The Spanish Civil War's major effect therefore came later in the drive toward "a republic of humanist intellectuals," which merged with the Martí tradition of appealing to political resurrection through the sufferings of *el pueblo*. Hennessey says Spain's appeal to the likes of Fidel lay in this rejection of the polite alliance of civilized *criollos* with *peninsulares* and *yanquis*. Modern technology came to be viewed as alien and inhuman.

Colombia and Peru (treated here by David Bushnell and Thomas Davies, Jr.) responded in two very "hispanic" ways. Colombia's nineteenth-century Spanish republicanism caused both Liberals and Conservatives of the 1930s to keep their distance from the Civil War's extremists, then collapsed into *La Violencia*. The ensuing political stalemate, based partly on the classic divisions of Hispanic society and partly on a genuine fear of restarting the bloodletting, is only too reminiscent of Spanish themes. In Peru, the Man on Horseback role so familiar in racially divided nations of the Hispanic world appeared in the "antipolitics" of the armed forces and set up an oscillation between military regimes of the right under General Benavides and of the left after 1968. The benevolent military despot banished by Spanish Republican sympathizers as a Habsburg and Bourbon import becomes in this context the sole hope for progress consistent with Hispanic traditions.

Chileans and Argentines of the 1930s lost their claim to be the Hispanic world's bridge to European democracy. According to Paul Drake, Chile's multi-ethnic cosmopolitanism, multiparty parliamentarianism, and reasonably rapid modernization were thought to make the nation immune to the polarities of Spanish thought. Hence, the major effect of the Civil War was to discourage intervention of the Chilean military to overturn the Popular Front's election victory in 1938. But instead of showing Spain the way back to democracy, Chile plunged toward the rivalries that brought Allende, then Pinochet to power in a seeming replay of Spain's tragedy. Argentines, as Falcoff pictures them,

meanwhile had to abandon the notion that the Napoleonic Code and British trade provided a way out of the choice between the *caudillismo* of Rosas and the *yanquismo* of Sarmiento. The replacement of "positivist presumption" by Perón's *justicialismo* and of Jacques Maritain's neo-Thomism by more rigidly orthodox church views created a nationalism whose main feature was the contention that opening the country to British trade and French law had destroyed Argentina's cottage industries and aborted a guided Latin democracy.

Frederick B. Pike's essay on the United States reaction to Franco amounts to a diatribe against "*id* people," such as Hemingway, for apostrophizing the Spanish Republicans as the last line of defense against heartless technological modernism. Pike's critique is not meant as a defense of *franquismo* but as a means of making way for the perspectives of John Quincy Adams, John C. Calhoun, and the Southern Agrarians of the 1930s—and for the "Catholic voluntarism" of the 1980s. He therefore views Franklin Roosevelt's "live-and-let-live approach" as an antidote to the civil religion of WASP materialism, an opening for "people not yet regarded as fully American," a major reason for neutrality in Spain's war, and the forerunner of Andrew Greeley's neoconservative pluralism.

Falcoff and Pike have assembled a series of remarkably scholarly essays, which really do hold a mirror up to economic and political modernization in the Americas. There is, however, ample room for criticism. First, might the overall picture not have been changed by an analysis of Venezuela, where a Hispanic tradition of leftist democracy has proved relatively strong, or for that matter of Puerto Rico, where Yankee materialism may actually have melded with *salsa* spirit? Second, is it wise to downplay the international dimension? European alliances and the Monroe Doctrine have been major influences on Spanish America during Spain's trauma and after. Third, do Falcoff and Pike hope to have cleared the way for a democratic *hispanismo* that accommodates both modernity and Spanish American traditions, perhaps on the model of post-Franco Spain? Many will claim that Spain and Spanish America, like their European cousins Italy and France, or like Venezuela, could well be moving toward a standard mixture of Christian Democrats and Social Democrats. *Hispanismo* might even eventually come to resemble the "vague and amorphous" civil religion of pluralism and utilitarianism that Pike deplores in the United States. In short, this book has by no means refuted the "positivist presumption" Falcoff attributes to Argentinian modernizers.

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Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

ALBERT BERGESEN, editor. *Crises in the World-System*. (Political Economy of the World-System Annuals, number 6.) Beverly Hills, Calif.: Sage. 1983. Pp. 311. Cloth \$25.00, paper \$12.50.

ALBERT BERGESEN, *Crises in the World-System: An Introduction*. IMMANUEL WALLERSTEIN, *Crises: The World-Economy, the Movements, and the Ideologies*. ROBERT PHILIP WEBER, *Cyclical Theories of Crises in the World-System*. ROBERT WUTHNOW, *Cultural Crises*. ALBERT BERGESEN, *Modeling Long Waves of Crisis in the World-System*. WILLIAM R. THOMPSON, *Succession Crises in the Global Political System: A Test of the Transition Model*. KATHRYN B. WARD, *The Economic Status of Women in the World-System: A Hidden Crisis in Development*. WALTER L. GOLDFRANK, *The Limits of Analogy: Hegemonic Decline in Great Britain and the United States*. PHILLIP E. HAMMOND, *Power Changes and Civil Religion: The American Case*. MARLENE DIXON, SUSANNE JONAS, and ED MCCAUGHAN, *Changes in the International Division of Labor and Low-Wage Labor in the United States*. KOSTIS PAPADANTONAKIS, *Mercantilism Inverted: World-Economic Crisis and Social Rectification in the United States in the 1980s*. RANDALL COLLINS, *The Weberian Revolution of the High Middle Ages*. MARK SELDEN, *Imposed Collectivization and the Crisis of Agrarian Development in the Socialist States*. ROBERT K. SCHAEFFER, *Crises of Supply: Two Cases of the Shortage of Seafaring Labor*. EDWARD L. KICK and JOSEPH CONATY, *African Economic Development: The Effects of East, West, and Chinese Penetration*. SING C. CHEW, *Capital in Crisis: Stagnation and Timber Company Operations, Canada, 1873-96*.

W. J. SHEILS, editor. *The Church and Healing*. (Studies in Church History, number 19.) Oxford: Basil Blackwell, for the Ecclesiastical History Society. 1982. Pp. xxiv, 440. £19.50.

TERENCE RANGER, Introduction. PEREGRINE HORDEN, *Saints and Doctors in the Early Byzantine Empire: The Case of*

Theodore of Sykeon. E. S. KARNOFSKY, *The Vision of Tainard miraculum de quodam canonico Guatenensi per Sanctum Donatianum Curato*. ANNE F. DAWTRY, *The modus medendi and the Benedictine Order in Anglo-Norman England*. EMMA MASON, "Recomadour in Quercy above All Other Churches": The Healing of Henry II. PETER BILLER, *Curate infirmos: The Medieval Waldensian Practice of Medicine*. RICHARD PALMER, *The Church, Leprosy, and Plague in Medieval and Early Modern Europe*. MICHAEL MACDONALD, *Religion, Social Change, and Psychological Healing in England, 1600-1800*. JOHN R. GUY, *Archbishop Secker as a Physician*. HENRY D. RACK, *Doctors, Demons, and Early Methodist Healing*. JUDITH F. CHAMP, *Bishop Milner, Holywell, and the Cure Tradition*. JOHN V. PICKSTONE, *Establishment and Dissent in Nineteenth-Century Medicine: An Exploration of Some Correspondence and Connections between Religious and Medical Belief-Systems in Early Industrial England*. G. P. CONNOLLY, *Little Brother Be at Peace: The Priest as Holy Man in the Nineteenth-Century Ghetto*. PETER J. LINEHAM, *Restoring Man's Creative Power: The Theosophy of the Bible Christians of Salford*. LOGIE BARROW, *Anti-Establishment Healing: Spiritualism in Britain*. BERNARD ASPINWALL, *Social Catholicism and Health: Dr. and Mrs. Thomas Low Nichols in Britain*. C. PETER WILLIAMS, *Healing and Evangelism: The Place of Medicine in Later Victorian Protestant Missionary Thinking*. A. F. WALLS, "The Heavy Artillery of the Missionary Army": The Domestic Importance of the Nineteenth-Century Medical Missionary. STUART MEWS, *The Revival of Spiritual Healing in the Church of England, 1920-26*. TERENCE RANGER, *Medical Science and Pentecost: The Dilemma of Anglicanism in Africa*. G. I. S. AMADI, *Healing in "The Brotherhood of the Cross and Star"*. R. L. STIRRAT, *Shrines, Pilgrimage, and Miraculous Powers in Roman Catholic Sri Lanka*. IRVING HEXHAM, *Some Aspects of Religion and Spiritual Healing in Cultsville, a Contemporary North American City*.

RICHARD JOHNSON *et al.*, editors. *Making Histories: Studies in History-Writing and Politics*. Foreword by MARY JO MAYNES. Minneapolis: University of Minnesota Press, for the Centre for Contemporary Cultural Studies. 1982. Pp. 379. Cloth \$35.00, paper \$15.95.

DAVID SUTTON, *Radical Liberalism, Fabianism, and Social History*. BILL SCHWARZ, "The People" in History: The

Communist Party Historians' Group, 1946–56. GREGOR MCLENNAN, E. P. Thompson and the Discipline of Historical Context. GREGOR MCLENNAN, Philosophy and History: Some Issues in Recent Marxist Theory. RICHARD JOHNSON, Reading for the Best Marx: History-Writing and Historical Abstraction. POPULAR MEMORY GROUP, Popular Memory: Theory, Politics, Method. MICHAEL BOMMES and PATRICK WRIGHT, "Charms of Residence": The Public and the Past. TRICIA DAVIS *et al.*, "The Public Face of Feminism": Early Twentieth-Century Writings on Women's Suffrage.

DAVID MCLELLAN, editor. *Marx: The First Hundred Years*. New York: St. Martin's. 1983. Pp. 316. \$19.95.

DAVID MCLELLAN, Introduction. RAYMOND WILLIAMS, Culture. VICTOR KIERNAN, History. TOM BOTTOMORE, Sociology. DAVID MCLELLAN, Politics. ERNEST MANDEL, Economics. ROY EDGLEY, Philosophy.

NORMAN FISCHER *et al.*, editors. *Continuity and Change in Marxism*. Atlantic Highlands, N.J.: Humanities Press or Harvester Press, Sussex. 1982. Pp. xii, 249. \$19.95.

MELVIN RADER, Marx's Three Worlds and Their Interrelation. STEFAN MORAWSKI, Marxism, Historicism, and the Philosophy of Art. NORMAN FISCHER, Economic Value, Ethics, and the Transition to Socialism. KOSTAS AXELOS, Theses on Marx. PHILIP GOLDSTEIN, Romanticism and Modernity in Lukácsian and Althusserian Marxism. MORTON G. WENGER, Lukács, Class Consciousness, and Rationality. WAYNE GRITTING, Midwives, Vanguard, and Class Consciousness. RONALD PERRIN, Marcuse and the Meaning of Radical Philosophy. JOSEPH C. FLAY, Habermas, Eurocommunism, and the Theory of Communication. N. GEORGIOPOULOS, Sartre and Alienation. CLEMENT MASLOFF, Lenin—Engineer of Revolution. CARL BOGGS, Gramsci and Eurocommunism. LOUIS PATSOURAS, Mao Zedong (Mao Tse-tung): The Long Road of Revolution to Communism. DOUGLAS KELLNER, Karl Korsch and Marxism.

SALLY M. MILLER, editor. *Flawed Liberation: Socialism and Feminism*. (Contributions in Women's Studies, number 19.) Westport, Conn.: Greenwood Press. 1981. Pp. xxiii, 214. \$27.50.

SALLY M. MILLER, Introduction. L. GLEN SERETAN, Daniel DeLeon and the Woman Question. SALLY M. MILLER, Women in the Party Bureaucracy: Subservient Functionaries. GRETCHEN KREUTER and KENT KREUTER, May Wood Simons: Party Theorist. MARI JO BUHLE, Lena Morrow Lewis: Her Rise and Fall. NEIL K. BASEN, The "Jennie Higginses" of the "New South in the West": A Regional Survey of Socialist Activists, Agitators, and Organizers, 1901–17. JOHN D. BUENKER, The Politics of Mutual Frustration: Socialists and Suffragists in New York and Wisconsin. WILLIAM C. PRATT, Women Socialists and Their Male Comrades: The Reading Experience, 1927–36.

WOLFGANG J. MOMMSEN and GERHARD HIRSCHFELD, editors. *Social Protest, Violence, and Terror in Nineteenth- and Twentieth-Century Europe*. New York: St. Martin's Press, for the German Historical Institute, London. 1982. Pp. xi, 411. \$25.00.

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PAUL THOMPSON, editor. *Our Common History: The Transformation of Europe*. Assisted by NATASHA BURCHARDT. Atlantic Highlands, N.J.: Humanities Press or Pluto Press, London. 1982. Pp. 334. \$19.95.

PAUL THOMPSON, Introduction. LUTZ NIETHAMMER, Oral History as a Channel of Communication between Workers and Historians. CRISTINA BORDERIAS and MERCEDES VILANOVA, Memories of Hope and Defeat: Catalan Miners and Fishermen under the Second Spanish Republic, 1931–39. LUISA PASSERINI, Work Ideology and Working Class Attitudes to Fascism. LILIANA LANZARDO, Class Consciousness and the Fiat Workers of Turin since 1943. DANIEL BERTAUX, Stories as Clues to Sociological Understanding: The Bakers of Paris. FAÏCH ELÉGOËT, The Peasant Economy of Léon, Brittany. GERHARD WILKE, Houses and People in a Village in Interwar Germany. DAGFINN SLETTAT, Farmwives, Farmhands, and the Changing Rural Community in Trøndelag, Norway. ANNA BRAVO, Italian Peasant Women

and the First World War. NELLEKE BAKKER and JAAP TALSMAN, Women and Work between the Wars: The Amsterdam Seamstresses. ISABELLE BERTAUX-WIAME, The Life History Approach to the Study of Internal Migration: How Women and Men Came to Paris between the Wars. LUCETTA SCARAFFIA, Marriage, Death, and Nature. DANIELE JALLA, The Working Class Family in Turin: Traditional Values and the Economy. EDVARD BULL, Industrial Boy Labour in Norway. ORVAR LÖFGREN, The Swedish Family: A Study of Privatisation and Social Change since 1880. ELMER LUCHTERHAND, Knowing and Not Knowing: Involvement in Nazi Genocide. ANNA MARIA BRUZZONE, Women in the Italian Resistance. DAVID ELLWOOD and ANNA BRAVO, Oral History and Resistance History in Italy. MARJAN SWEGMAN, Women and Resistance Organisations in the Netherlands. PAUL THOMPSON, The Humanistic Tradition and Life Histories in Poland. SVEN LINDQVIST, "Dig Where You Stand."

PETER N. STEARNS, editor. *Old Age in Preindustrial Society*. New York: Holmes and Meier. 1982. Pp. 280. \$29.75.

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3. FREQUENCY OF ISSUE Five times per year - Feb., Apr., Jun., Oct., & Dec.	3A. NO. OF ISSUES PUBLISHED ANNUALLY Five	3B. ANNUAL SUBSCRIPTION PRICE Class 1 \$43.00
4. COMPLETE MAILING ADDRESS OF KNOWN OFFICE OF PUBLICATION (Street, City, Country, State and ZIP Code) (Not printers) 400 A Street, S.E. Washington, D.C. 20003		
5. COMPLETE MAILING ADDRESS OF THE HEADQUARTERS OF GENERAL BUSINESS OFFICES OF THE PUBLISHER (Not printers) 400 A Street, S.E. Washington, D.C. 20003		
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Index to *American Historical Review*, Volume 88

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- "Aaron Burr: The Conspiracy and Years of Exile, 1805–1836," by Lomask, 1062
 Abadan-Unat, Nermin, *et al.*, editors, "Women in Turkish Society," 1031
 "The Abbey of St. Germain des Prés in the Seventeenth Century," by Ultee, 123
 Abbott, Carl (R), 199
 "Abélard en son temps," edited by Jolivet (E), 233
 Abir, Mordechai, "Ethiopia and the Red Sea: The Rise and Decline of the Solomonic Dynasty and Muslim-European Rivalry in the Region," 727
 "The Abraham Lincoln Encyclopedia," by Neely, 754
 "Abraham Lincoln," by Anderson, 184
 "Abraham Went Out: A Biography of A. J. Muste," by Robinson, 206
 Abraham, David (C), 1145
 Abrahamian, Ervand, "Iran between Two Revolutions," 1303
 Abrahamson, James L. (R), 1094
 Abzug, Robert H. (R), 761
 "Academies in Ming China," by Meskill, 450
 Ackerman, Evelyn Bernette (R), 995
 "Across the Border: Rural Development in Mexico and Recent Migration to the United States," by Cross and Sandos, 226
 Acuña, Rodolfo (R), 504
 Adam, Heribert (R), 965
 "Adam Smith," by Campbell and Skinner, 977
 Adams, Henry, 1077
 Adams, John Quincy, 179
 Adams, Marian Hooper, 196
 Adams, R. J. Q. (R), 118
 Adams, Willi Paul (R), 467
 Adanir, Fikret, "Die Makedonische Frage: Ihre Entstehung und Entwicklung bis 1908," 1019
 "Adel og embede: Embedsfordeling og karrieremobilitet hos den dansk-norske adel, 1588–1660," by Solter, 1279
The Adjustment of Italian Immigrants in Buenos Aires and New York, 1870–1914, by Bailly, 281–305
 "Adjustment to Empire: The New England Colonies, 1675–1715," by Johnson, 462
 Adler, Philip J. (R), 1294
 "Administration et parlement depuis 1815," by Bruguière *et al.* (E), 1122
 "Admiral von Hipper," by Philbin, 420
 Adorno, Theodor, 1245
 Africa, Thomas W. (R), 660
 "African Women and the Law," edited by Hay and Wright (E), 1123
 "The Afro-Yankees: Providence's Black Community in the Antebellum Era," by Cottrol, 1324
 "After the Gold Rush: Society in Grass Valley and Nevada City, California, 1849–1870," by Mann, 1326
 "Against All Odds: The Feminist Movement in Mexico to 1940," by Macías, 507
 AGRARIAN HISTORY: Cassanelli, "The Shaping of Somali Society," 1038; Connolly, "Priests and People in Pre-Famine Ireland, 1780–1845," 400; Corni, "Stato assoluto e società agraria in Prussia nell'età di Federico II," 1285; Cushner, "Farm and Factory: The Jesuits and the Development of Agrarian Capitalism in Colonial Quito, 1600–1767," 1350; Dethloff and May, editors, "Southwestern Agriculture," 758; Dodgshon, "Land and Society in Early Scotland," 376; Dyson, "Red Harvest: The Communist Party and American Farmers," 208; Hooglund, "Land and Revolution in Iran, 1960–1980," 1033; Lachiver, "Vin, vigne, et vigneron en région parisienne du XVII^e au XIX^e siècle," 995; Le Roy Ladurie and Goy, "Tithe and Agrarian History from the Fourteenth to the Nineteenth Centuries," 1240; McGregor, "Counting Sheep: From Open Range to Agribusiness on the Columbia Plateau," 1087; Poni, "Fossi e cavedagne benedicon le campagne," 708; Shaffer, "Family and Farm: Agrarian Change and Household Organization in the Loire Valley, 1500–1900," 686
 Akenson, Donald H. (R), 400
 Akers, Charles W., "The Divine Politician: Samuel Cooper and the American Revolution in Boston," 1320
 Aksakov, K. S., 1026
 "Alain Locke," edited by Linnemann (E), 1124
 "Alaskan John G. Brady," by Hinckley, 1328
 Alberigo, Giuseppe, "Chiesa conciliare: Identità e significato del conciliarismo," 93
 "Albert Einstein: Historical and Cultural Perspectives," edited by Holton and Elkana (E), 514
 Albert, Peter J., and Ronald Hoffman, editors, "Sovereign States in an Age of Uncertainty," 1061
 Albinski, Henry S. (R), 85
 Alcott, Amos Bronson, 761
 Aldcroft, D. H. (R), 1261
 Alekseev, Iu. G., "Pskovskaia Sudnaia gramota i ee vremia: Razvitie feodal'nykh otnoshenii na Rusi XIV–XV vv.," 145
 "Alexander the Great," by Hammond, 1252
 Alexander, Thomas B. (R), 476
 Alexander, Thomas G. (R), 759
 "Alien Merchants in England in the High Middle Ages," by Lloyd, 661
 "All Clever Men, Who Make Their Way: Critical Discourse in the Old South," edited by O'Brien (E),

- 791
 Allardyce, Gilbert (R), 75
 Allen, Gay Wilson, "Waldo Emerson: A Biography," 182
 Allen, James Smith (C), 807
 Allen, James Smith (R), 129
 Allin, Craig W., "The Politics of Wilderness Preservation," 759
 Allis, Frederick S., Jr., and Philip Chadwick Foster Smith, editors, "Sibley's Heir: A Volume in Memory of Clifford Kenyon Shipton" (E), 232
 Allison, David Kite, "New Eye for the Navy: The Origin of Radar at the Naval Research Laboratory," 210
 Alpern, Mildred (C), 246
 Alpert, Michael, "La reforma militar de Azaña, 1931–1933," 1002
 "Alternative America," by Thomas, 1330
 Altholz, Josef L. (R), 397
 Altschul, Michael (R), 662
 Alulã, Räs, 1037
 "Ambivalent Friends: Afro-Americans View the Immigrant," by Shankman, 768
 Ambrosini, Federica, "Paesi e mari ignoti: America e colonialismo europeo nella cultura veneziana, secoli XVI–XVII," 1017
 "America and the Survivors of the Holocaust," by Dinnerstein, 215
 "America and the Winter War, 1939–1940," by Jacobs, 210
 "La America: The Sephardic Experience in the United States," by Angel, 1331
 "The American Experiment: The Vineyard of Liberty," by Burns, 460
 AMERICAN INDIAN HISTORY: Burt, "Tribalism in Crisis: Federal Indian Policy, 1953–1960," 1107; Coel, "Chief Left Hand," 479; Cook, "Demographic Collapse: Indian Peru, 1520–1620," 509; Dippie, "The Vanishing American: White Attitudes and U.S. Indian Policy," 744; Fowler, "Arapaho Politics, 1851–1978," 480; Green, "The Politics of Indian Removal: Creek Government and Society in Crisis," 478; Iverson, "Carlos Montezuma and the Changing World of American Indians," 1078; Jones, "License for Empire: Colonialism by Treaty in Early America," 747; Lawson, "Damned Indians: The Pick-Sloan Plan and the Missouri River Sioux," 500; Milner, "With Good Intentions: Quaker Work among the Pawnees, Osos, and Omahas," 1078; Pagden, "The Fall of Natural Man: The American Indian and the Origins of Comparative Ethnology," 1114; Peroff, "Menominee Drums: Tribal Termination and Restoration," 501; Ruby and Brown, "Indians of the Pacific Northwest," 479; Salisbury, "Manitou and Providence: Indians, Europeans, and the Making of New England," 746; Stern, "Peru's Indian Peoples and the Challenge of Spanish Conquest: Huamanga to 1640," 508; Tiller, "The Jicarilla Apache Tribe," 1332; Trafzer, "The Kit Carson Campaign: The Last Great Navajo War," 479
 "The American Inquisition," by Kutler, 1104
 "American Intervention in Greece, 1943–1949," by Wittner, 213
 "American Labor in the Southwest," edited by Foster, 200
 "American Legal Culture, 1908–1940," by Johnson, 203
 "The American Magic: Codes, Ciphers, and the Defeat of Japan," by Lewin, 211
 "American Profile, 1900–1909," by Wagenknecht, 490
 "Amos Bronson Alcott," by Dahlstrand, 761
 Ampalavanar, Rajeswary, "The Indian Minority and Political Change in Malaya, 1945–1957," 458
 "Analyzing Electoral History," by Clubb *et al.*, 168
 "Anarchist Women, 1870–1920," by Marsh, 1330
 "The Anarchists of Casa Viejas," by Mintz, 1276
 "Anatomy of a Lynching: The Killing of Claude Neal," by McGovern, 772
 Anderson, Arlow W. (R), 487
 Anderson, Dwight G., "Abraham Lincoln: The Quest for Immortality," 184
 Anderson, Peter D., "Robert Stewart, Earl of Orkney, Lord of Shetland, 1533–1593," 989
 Andrew, Donna T. (R), 1262
 Andrews, William G., "Presidential Government in Gaullist France: A Study of Executive-Legislative Relations, 1958–1974," 692
 Andrzejewski, Marek, "Socjaldemokratyczna partia wolnego miasta Gdańska, 1920–36," 437
 Angel, Marc D., "La America: The Sephardic Experience in the United States," 1331
The Angevin Strategy of Castle Building in the Reign of Fulk Nerra, by Bachrach, 533–60
 "Anglo-Japanese Alienation, 1919–1952," edited by Nish (E), 786
 ANTHROPOLOGY: Leopold, "Culture in Comparative and Evolutionary Perspective," 649; Stenson, "Class, Race, and Colonialism in West Malaysia," 1311; Vincent, "Teso in Transformation," 1305
 "The Anti-Masonic Party in the United States, 1826–1843," by Vaughn, 1064
 Antoljak, Stjepan, "Pacta ili Concordia od 1102. godine," 103
 "An Anxious Democracy: Aspects of the 1830s," by Duffy and Muller, 1063
 "Apocalyptic and Merkavah Mysticism," by Gruenwald, 371
 Appadorai, A., "The Domestic Roots of India's Foreign Policy, 1947–1972," 162
 "L'appel à l'Afrique," by Michel, 728
 Aptheker, Bettina, "Woman's Legacy: Essays on Race, Sex, and Class in American History," 1056
 "The Arab Balance of Power," by Taylor, 725
 "Arapahoe Politics, 1851–1978," by Fowler, 480
 "Arbeiter in Deutschland," edited by Langewiesche and Schönhoven (E), 234
 "The Archaeological and Linguistic Reconstruction of African History," edited by Ehret and Posnansky (E), 1357
 ARCHAEOLOGY AND NUMISMATICS: Butt and Donnachie, "Industrial Archaeology in the British Isles," 114; Clark, editor, "The Cambridge History of Africa," vol. 1: "From the Earliest Times to c. 500 B.C.," 726; Fedorov-Davydov, "Monety Moskovskoi Rusi," 144; Hume, "Martin's Hundred," 170; Zhongshu, "Han Civilization," 152
 ARCHITECTURE AND LANDSCAPE HISTORY: Baker, "Richard Morris Hunt," 195; Cranz, "The Politics of Park Design," 1334; Hayden, "The Grand Domestic Revolution: A History of Feminist Designs for American Homes, Neighborhoods, and Cities," 1080; Hitchcock, "German Renaissance Architecture," 416; Hosmer, "Preservation Comes of Age: From Williamsburg to the National Trust," 1100; Irving, "Indian Summer," 738; Kubler, "Building the Escorial," 410
 Arendt, Hannah, 75
 Arlinghaus, Bruce E., editor, "Arms for Africa: Military Assistance and Foreign Policy in the Developing World" (E), 790
 "Armies and Warfare in Europe, 1648–1789," by Childs, 1242
 Arnitage, Susan H. (R), 184

- "Arms for Africa: Military Assistance and Foreign Policy in the Developing World," edited by Arlinghaus (E), 790
- Armstrong, David A., "Bullets and Bureaucrats: The Machine Gun and the United States Army, 1861–1916," 1094
- Armstrong, David, "The Rise of the International Organisation: A Short History," 1250
- Armstrong, John A., "Nations before Nationalism," 650
- Armstrong, John, Jr., 470
- Armstrong, William M. (R), 474
- "Army, Aristocracy, Monarchy: Essays on War, Society, and Government in Austria, 1618–1780," by Barker, 425
- "Army Generals and Reconstruction," by Dawson, 186
- Arnesen, Peter J. (R), 1046
- Arnheim, M. T. W. (R), 659
- Arnstein, Walter L., "Protestant versus Catholic in Mid-Victorian England: Mr. Newdegate and the Nuns," 113
- "Art and Eloquence in Byzantium," by Maguire, 380
- "Art and Politics in the Weimar Period," by Willett, 420
- "Art and Society: The New Art Movement in Vienna, 1897–1914," by Shedel, 426
- ART HISTORY: Havens, "Artist and Patron in Postwar Japan," 735; Marling, "Wall-to-Wall America: A Cultural History of Post-Office Murals in the Great Depression," 775; Podro, "The Critical Historians of Art," 1244
- "The Art of Travel," edited by Dodd (E), 1120
- "Artabados, der rechtgläubige Vorkämpfer der göttlichen Lehren," by Speck, 94
- Artaud, Denise, "La question des dettes interalliées et la reconstruction de l'Europe (1917–1929)," 106
- "Arthur Lee," by Potts, 748
- "Artisans and Guild Life in the Later Safavid Period," by Keyvani, 1302
- "Artist and Patron in Postwar Japan," by Havens, 735
- "Artists and Artisans in Delft," by Montuás, 411
- Artola, Miguel, "La hacienda del Antiguo Régimen," 1000
- "At Dawn We Slept: The Untold Story of Pearl Harbor," by Prange, 496
- Athanassiadi-Fowden, Polymnia, "Julian and Hellenism: An Intellectual Biography," 90
- Atkins, G. Pope (R), 503
- "Attack and Die: Civil War Military Tactics and the Southern Heritage," by McWhiney and Jamieson, 475
- "Aufklärung und Revolution," by Schleich, 402
- Augustinos, Gerasimos (R), 141
- "Auslandsdeutschtum und Drittes Reich," by Volberg, 230
- Ausmus, Harry J., "The Polite Escape: On the Myth of Secularization," 358
- Auspitz, Katherine (R), 687
- Auspitz, Katherine, "The Radical Bourgeoisie: The Ligue de l'Enseignement and the Origins of the Third Republic, 1866–1885," 128
- Austen, Ralph A. (R), 1040
- "Austria's Eastern Question, 1700–1790," by Roider, 711
- "Authoritarian Politics in a Transitional State," by Batkay, 715
- "Authoritarian Socialism in America," by Lipow, 194
- AUTOBIOGRAPHIES, DIARIES, AND MEMOIRS: Link *et al.*, "The Papers of Woodrow Wilson," vols. 36–38, 1091; MacKenzie and MacKenzie, editors, "The Diary of Beatrice Webb," vol. 1: "Glitter Around and Darkness Within," 1873–1892," 984; McLean, "Papers concerning Robertson's Colony in Texas," vol. 9: "October 1834 through March 20, 1835," 1322; Schlissel, "Women's Diaries of the Westward Journey," 184; Taylor *et al.*, "Diary of John Quincy Adams," 179
- "Autocracy and Aristocracy: The Russian Service Elite of 1730," by Meehan-Waters, 437
- "The Automobile Revolution," by Bardou *et al.*, 86
- Avesani, Rino, *et al.*, editors, "Società, politica e cultura a Carpi ai tempi di Alberto III Pio" (E), 235
- Avni, Haim, "Spain, the Jews, and Franco," 409
- Avrekh, A. Ia., "Tsarizm i IV Duma, 1912–1914 gg.," 1029
- Avrich, Paul (R), 1243
- Ayçoberry, "Cologne entre Napoléon et Bismarck: La croissance d'une ville rhénane," 418
- Babcock, Robert H. (R), 1111
- Babikova, E. N., "Dvoevlastie v Sibiri," 719
- Bachrach, Bernard S. (R), 1256
- Bachrach, Bernard S., *The Angevin Strategy of Castle Building in the Reign of Fulk Nerra, 987–1040*, 533–60
- Bacigalupo, Marvyn Helen, "A Changing Perspective: Attitudes toward Creole Society in New Spain, 1521–1610," 1113
- Backus, Charles, "The Nan-chao Kingdom and T'ang and China's Southwestern Frontier," 457
- Bade, Klaus J., editor, "Imperialismus und Kolonialmission: Kaiserliches Deutschland und koloniales Imperium," 1011
- Baechler, Christian, "Le parti catholique alsacien, 1890–1939: Du Reichsland à la République jacobine," 1274
- Baer, Gabriel, "Fellah and Townsman in the Middle East: Studies in Social History," 723
- Baer, George W. (R), 1250
- Bahmueller, Charles F., "The National Charity Company: Jeremy Bentham's Silent Revolution," 979
- Bailey, David C. (R), 505
- Bailey, Kenneth K. (R), 1096
- Baillargeon, Noël, "Le Séminaire de Québec de 1760 à 1800," 1348
- Baily, Samuel L., *The Adjustment of Italian Immigrants in Buenos Aires and New York, 1870–1914*, 281–305
- Bak, János M., and Béla K. Király, editors, "From Hunyadi to Rákóczi: War and Society in Late Medieval and Early Modern Hungary" (E), 1123
- Baker, Alan R. H., and Mark Billinge, editors, "Period and Place: Research Methods in Historical Geography" (E), 785
- Baker, Sir Herbert, 738
- Baker, Paul R., "Richard Morris Hunt," 195
- Baker, Robert L. (R), 1256
- Bakewell, Peter (R), 509
- Bakunin, Mikhail, 1243
- "Balance of Intrigue," by Lensen, 1306
- Balderrama, Francisco E., "In Defense of La Raza: The Los Angeles Mexican Consulate and the Mexican Community, 1929 to 1936," 774
- Baldini, A. Enzo, "Puntigli spagnoleschi e intrighi politici nella Roma di Clemente VIII: Girolamo Frachetta e la sua relazione del 1603 sui cardinali," 709
- "Baldur von Schirach: Hitlers Jugendführer," by Wortmann, 1288
- "Balkan Economic History, 1550–1950," by Lampe, 139
- "Der Balkan in deutschen und österreichischen

- Reise- und Erlebnisberichten, 1912–1918," by Golczewski, 1294
- "Baltimore: The Nineteenth-Century Black Capital," by Graham, 1324
- Balán, Jorge, *Comment on Klein, The Integration of Italian Immigrants into the United States and Argentina* (Forum), 330–34
- "Ban Johnson: Czar of Baseball," by Murdock, 1095
- Bance, A. F., editor, "Weimar Germany: Writers and Politics" (E), 1122
- "A Band of Prophets: The Vanderbilt Agrarians," edited by Havard and Sullivan (E), 235
- Banks, J. A., "Victorian Values: Secularism and the Size of Families," 395
- Banks, Olive, "Faces of Feminism: A Study of Feminism as a Social Movement," 83
- Bankwitz, Philip C. F. (R), 999
- "Les banques, l'état et le marché immobilier en France à l'époque contemporaine, 1820–1940," by Lescure, 690
- Barber, John, "Soviet Historians in Crisis, 1928–1932," 148
- Barbier, Jacques A. (R), 223
- Bardou, Jean-Pierre *et al.*, "The Automobile Revolution: The Impact of an Industry," 86
- Barker, Thomas M. (R), 426
- Barker, Thomas M., "Army, Aristocracy, Monarchy: Essays on War, Society, and Government in Austria, 1618–1780," 425
- Barkun, Michael (R), 962
- Barnes, Timothy D., "The New Empire of Diocletian and Constantine," 660
- Barnwell, John, "Love of Order: South Carolina's First Secession Crisis," 473
- Barracrough, Geoffrey, "From Agadir to Armageddon: Anatomy of a Crisis," 1247
- Barratt, Glynn, "Russophobia in New Zealand, 1838–1908," 1313
- Barrier, N. Gerald, editor, "The Census in British India: New Perspectives," 737
- "Barros Arana's *Historia general de Chile*," by Yeager, 229
- Barrv, James, 980
- Barstow, Anne Llewellyn, "Married Priests and the Reforming Papacy: The Eleventh-Century Debates," 660
- Barth, Gunther (R), 757, 1326
- Barlett, Robert, "Gerald of Wales, 1146–1223," 968
- "Bartolomé Mitre," by Robinson, 1119
- Bartram, John, 175
- Basch, Norma, "In the Eyes of the Law: Women, Marriage, and Property in Nineteenth-Century America," 485
- "Basileus: The Evidence for Kingship in Geometric Greece," by Drews, 1251
- Batkay, William M., "Authoritarian Politics in a Transitional State: Istvan Bethlen and the Unified Party in Hungary, 1919–1926," 715
- Baumgart, Winfried, "Imperialism: The Idea and Reality of British and French Colonial Expansion, 1880–1914," 667
- "Baunkohlenbergbau und Arbeiterbewegung," by Englisch, 1021
- Baur, John E. (R), 1068
- Bawa, Vasant Kumar, "Latin American Integration," 224
- Baxter, Stephen B., editor, "England's Rise to Greatness, 1660–1763" (E), 1121
- Bavat, Mangol (R), 1033
- Bayat, Mangol, "Mysticism and Dissent: Socioreligious Thought in Qajar Iran," 723
- Bayley, Edwin R., "Joe McCarthy and the Press," 1104
- "Beautiful Merchandise," by Gronewold, 1044
- Bebbington, D. W., "The Nonconformist Conscience: Chapel and Politics, 1870–1914," 111
- Becker, Marvin B. (R), 379
- Bedeski, Robert E., "State-Building in Modern China: The Kuomintang in the Prewar Period," 450
- Beecher, Henry Ward, 484
- Beer, Barrett L., "Rebellion and Riot: Popular Disorder in England during the Reign of Edward VI," 670
- Beer, John J. (R), 417
- Beezlev, William H. (R), 1351
- Behlmer, George K., "Child Abuse and Moral Reform in England, 1870–1908," 1268
- Beidelman, T. O., "Colonial Evangelism: A Socio-Historical Study of an East African Mission at the Grassroots," 729
- Beisner, Robert L. (R), 1334
- Belknap, Michal R. (R), 777
- Bell, Rudolph M., and Donald Weinstein, "Saints and Society: The Two Worlds of Western Christendom, 1000–1700," 1255
- Bell, Rudolph, M. (R), 708
- Bellamy, Edward, 194, 1330
- Bender, Thomas (R), 167
- Benecke, Gerhard (R), 1007
- Benecke, Gerhard, "Maximilian I (1459–1519): An Analytical Biography," 1289
- "Benevolent Assimilation: The American Conquest of the Philippines, 1899–1903," by Miller, 1334
- Benjamin, Walter, 1245
- Bennett, Edward W. (R), 1286
- Bennett, James (C), 524
- Bennett, James (R), 116
- Bennett, Norman R. (R), 150
- Bentham, Jeremy, 979
- Berdyaev, Nicholas, 1245
- Berend, Iván T., and György Ránki, "The European Periphery and Industrialization, 1780–1914," 975
- Berger, David (R), 93
- Berger, Iris, "Religion and Resistance: East African Kingdoms in the Precolonial Period," 150
- Bergesen, Albert, editor, "Crises in the World-System" (E), 1355
- Berkeley, Edmund, and Dorothy Smith Berkeley, "The Life and Travels of John Bartram: From Lake Ontario to the River St. John," 175
- Berlin, Ira, and Herbert G. Gutman, *Natives and Immigrants, Free Men and Slaves: Urban Workingmen in the Antebellum South, 1175–1200*
- Berman, Morris, "The Reenchantment of the World," 359
- Bernhard, Winfred E. A. (R), 1061
- Bernstein, Eduard, 561–78
- Berridge, Virginia, and Griffith Edwards, "Opium and the People: Opiate Use in Nineteenth-Century England," 396
- Berry, Mary Elizabeth, "Hideyoshi," 1046
- Berry, Mary F. (R), 469
- Berry, Mary Frances, and John W. Blassingame, "Long Memory: The Black Experience in America," 1342
- Berryman, Jack W. (R), 1095
- Berthrong, Donald J. (R), 480
- Besier, Gerhard, "Krieg—Frieden—Abrüstung: Die Haltung der europäischen und amerikanischen Kirchen zur Frage der deutschen Kriegsschuld, 1914–1933—Ein kirchenhistorischer Beitrag zur Friedensforschung und Friedenserziehung," 703
- Best, Gary Dean, "To Free a People: American Jewish Leaders and the Jewish Problem in Eastern Europe, 1890–1914," 198

- Best, Geoffrey (R), 366
 Best, Geoffrey, "War and Society in Revolutionary Europe, 1770–1870," 973
 Bethlen, Istvan, 715
 "A Better World: The Great Schism, Stalinism, and the American Intellectuals," by O'Neill, 1337
 Betts, Raymond F. (R), 446
 "Between Science and Values," by Graham, 359
 Bew, Paul (R), 121
 Bew, Paul, and Henry Patterson, "Seán Lemass and the Making of Modern Ireland, 1945–66," 1272
 "Beware the People Weeping: Public Opinion and the Assassination of Abraham Lincoln," by Turner, 1070
 "Beyond Separate Spheres: Intellectual Roots of Modern Feminism," by Rosenberg, 484
 "Beyond the Hiss Case," edited by Theoharis, 1105
 Bezucha, Robert (R), 418
 "The Bible in America," edited by Hatch and Noll, 744
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 Bicha, Karel D. (R), 208
 Billias, George A. (R), 752
 Billinge, Mark, and Alan R. H. Baker, editors, "Period and Place: Research Methods in Historical Geography" (E), 785
 Billings, Warren M. (C), 530
 Bingham, Alfred, 471
 Bingham, Edwin R. (R), 756
 Bingham, Hiram, 471
 Bingham, Stephen, 471
 BIOGRAPHY: Akers, "The Divine Politician: Samuel Cooper and the American Revolution in Boston," 1320; Allen, "Waldo Emerson," 182; Alton, "Dwight D. Eisenhower," 215; Anderson, "Abraham Lincoln," 184; Anderson, "Robert Stewart, Earl of Orkney," 989; Athanassiadi-Fowden, "Julian and Hellenism," 90; Bartlett, "Gerald of Wales, 1146–1223," 968; Bayley, "Joe McCarthy and the Press," 1104; Benecke, "Maximilian I (1459–1519)," 1289; Berkeley and Berkeley, "The Life and Travels of John Bartram," 175; Berry, "Hideyoshi," 1046; Bew and Patterson, "Seán Lemass and the Making of Modern Ireland, 1945–66," 1272; Blantz, "A Priest in Public Service: Francis J. Haas and the New Deal," 1099; Bourne, "Palmerston: The Early Years, 1784–1841," 677; Bregman, "Synesius of Cyrene," 369; Burmeister, "Prince Philipp Eulenburg-Hertefeld, 1847–1921," 130; Campbell and Skinner, "Adam Smith," 977; Canny, "The Upstart Earl: A Study of the Social and Mental World of Richard Boyle," 992; Caro, "The Years of Lyndon Johnson," vol. 1: "The Path to Power," 1346; Chalfant, "Both Sides of the Ocean: A Biography of Henry Adams," 1077; Clark, "The Miners' Fight for Democracy," 1096; Coel, "Chief Left Hand," 479; Cooley, "T. F. Wade in China," 1043; Craddock, "Young Edward Gibbon," 110; Cross, "Lord Swinton," 1271; Dahlstrand, "Amos Bronson Alcott," 761; Dean, "Defender of the Race: James Theodore Holly, Black Nationalist Bishop," 1073; Demaitre, "Doctor Bernard de Gordon," 663; Dugger, "The Politician: The Life and Times of Lyndon Johnson, the Drive for Power," 502; Duram, "A Moderate among Extremists: Dwight D. Eisenhower and the School Desegregation Crisis," 1345; Edwards, "Pat McCarran," 1099; Epstein, "The Lion of Freedom: Feargus O'Connor and the Chartist Movement, 1832–1842," 393; Erlich, "Ethiopia and Eritrea during the Scramble for Africa," 1037; Estes, "Christian Magistrate and State Church: The Reforming Career of Johannes Brenz," 1006; Ferrell, "Harry S. Truman and the Modern American Presidency," 1344; Fichtner, "Ferdinand I of Austria," 424; Flower, "John Dickinson: Conservative Revolutionary," 1321; Fry, "Henry S. Sanford," 1075; Gailey, "Lugard and the Abokuta Uprising," 1036; George, "John Robinson and the English Separatist Tradition," 1263; Greenhalgh, "Pompey: The Republican Prince," 656; Greenhalgh, "Pompey: The Roman Alexander," 656; Greenstein, "The Hidden-Hand Presidency: Eisenhower as Leader," 1345; Hammond, "Alexander the Great," 1252; Hinckley, "Alaskan John G. Brady," 1328; Holt, "Manning Clark and Australian History, 1915–1963," 742; Hopkins, "A Woman to Deliver Her People: Joanna Southcott and English Millenarianism in an Era of Revolution," 392; Hunt, "Sailor-Scholar: Admiral Sir Herbert Richmond," 987; Iverson, "Carlos Montezuma and the Changing World of American Indians," 1078; Jacobs, "Borodin: Stalin's Man in China," 1029; Jones, "John A. Logan," 1073; Kaledin, "The Education of Mrs. Henry Adams," 196; Katz, "Herbert Marcuse and the Art of Liberation," 963; Kelly, "Mikhail Bakunin: A Study in the Psychology and Politics of Utopianism," 1243; Knecht, "Francis I," 122; Kotje and Zimmermann, editors, "Hrabanus Maurus," 969; Lander, "The Calhoun Family and Thomas Green Clemson: The Decline of a Southern Patriarchy," 1323; Lear, "Harold L. Ickes," 206; Lennon, "Richard Stanishurst," 991; Lewsen, "John X. Merriman," 730; Link, editor, "Woodrow Wilson and a Revolutionary World, 1913–1921," 765; Locke, "A Fantasy of Reason: The Life and Thought of William Godwin," 392; Lomask, "Aaron Burr: The Conspiracy and Years of Exile, 1805–1836," 1062; Lyons and Jackson, "Saladin," 721; Macdougall, "James III," 662; Madden and Hamilton, "Freedom and Grace: The Life of Asa Mahan," 1323; McMurray, "George Washington Carver," 197; Miller, "Fathers and Sons: The Bingham Family and the American Mission," 471; Miller, "Governor Philip F. La Follette, the Wisconsin Progressives, and the New Deal," 1097; Moody, "Davitt and Irish Revolution, 1846–82," 401; Mosley, "Marshall: Hero for Our Times," 776; Murdock, "Ban Johnson: Czar of Baseball," 1095; Murphy, "Charles Gravier, Comte de Vergennes," 996; Murrah, "C. C. Slaughter," 1074; Nagel, "Descent from Glory: Four Generations of the John Adams Family," 1075; O'Higgins, "Yves de Vallone," 699; Oates, "Let the Trumpet Sound: The Life of Martin Luther King, Jr.," 1108; Overton, "Perkins/Budd: Railway Statesmen of the Burlington," 1086; Philbin, "Admiral von Hipper," 420; Potts, "Arthur Lee," 748; Pressly, "The Life and Art of James Barry," 980; Preussen, "John Foster Dulles," 217; Reeves, "Lancastrian Englishmen," 98; Richards, "Consul of God," 92; Robinson, "Abraham Went Out: A Biography of A. J. Muste," 206; Robinson, "Bartolomé Mitre," 1119; Röhl and Sombart, editors, "Kaiser Wilhelm II," 701; Salvatore, "Eugene V. Debs," 1336; Saunders, "Edward Jenner: The Cheltenham Years," 676; Serle, "John Monash," 1054; Shannon,

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 Bireley, Robert (R), 698
 Birley, A. R. (R), 967
 Birn, Raymond (R), 687
 Birnbaum, Max P., "Staat und Synagoge, 1918–1938: Eine Geschichte des Preussischen Landesverbandes jüdischer Gemeinden, 1918–1938," 422
 "Birth Control and the Population Question in England, 1877–1930," by Soloway, 680
 "The Birth of a Consumer Society," by McKendrick *et al.*, 977;
 "Bitter Legacy: Polish-American Relations in the Wake of World War II," by Lukas, 1103
 Bitton, Davis (R), 402
 Black, J. L. (R), 1299
 "Black Americans and the Missionary Movement in Africa," edited by Jacobs (E), 791
 BLACK AND AFRO-AMERICAN HISTORY: Aptheker, "Woman's Legacy: Essays on Race, Sex, and Class in American History," 1056; Berry and Blassingame, "Long Memory: The Black Experience in America," 1342; Bodnar *et al.*, "Lives of Their Own: Blacks, Italians, and Poles in Pittsburgh, 1900–1960," 1089; Bringhurst, "Saints, Slaves, and Blacks: The Changing Place of Black People within Mormonism," 189; Cottrol, "The Afro-Yankees: Providence's Black Community in the Antebellum Era," 1324; Cox, "Lincoln and Black Freedom," 473; Dean, "Defender of the Race: James Theodore Holly, Black Nationalist Bishop," 1073; Drago, "Black Politicians and Reconstruction in Georgia," 1071; Field, "The Politics of Race in New York: The Struggle for Black Suffrage in the Civil War Era," 755; Graham, "Baltimore: The Nineteenth-Century Black Capital," 1324; Harris, "The Harder We Run: Black Workers since the Civil War," 768; McGovern, "Anatomy of a Lynching: The Killing of Claude Neal," 772; Oates, "Let the Trumpet Sound: The Life of Martin Luther King, Jr.," 1108; Rabinowitz, editor, "Southern Black Leaders of the Reconstruction Era," 1071; Russell-Wood, "The Black Man in Slavery and Freedom in Colonial Brazil," 1117; Shankman, "Ambivalent Friends: Afro-Americans View the Immigrant," 768; Walker, "A Rock in a Weary Land: The African Methodist Episcopal Church during the Civil War and Reconstruction," 476
 "The Black Man in Slavery and Freedom in Colonial Brazil," by Russell-Wood, 1117
 "Black Politicians and Reconstruction in Georgia," by Drago, 1071
 Blackford, Mansel G., "A Portrait Cast in Steel: Buckeye International and Columbus, Ohio, 1881–1980," 1085
 Blackton, Charles S. (R), 163
 Blake, Robert, "Disraeli's Grand Tour: Benjamin Disraeli and the Holy Land, 1830–31," 981
 Blanchard, Peter, "The Origins of the Peruvian Labor Movement, 1883–1919," 1118
 Blantz, Thomas E., "A Priest in Public Service: Francis J. Haas and the New Deal," 1099
 Blassingame, John W., and Mary Frances Berry, "Long Memory: The Black Experience in America," 1342
 Blaszk, Barbara J. (R), 120
 Blessing, Werner K., "Staat und Kirche in der Gesellschaft: Institutionelle Autorität und mentaler Wandel in Bayern während des 19. Jahrhunderts," 1010
 Bliss, Michael, "The Discovery of Insulin," 964
 "Blue-Collar Workers in Eastern Europe," edited by Triska and Gati, 140
 Blumenthal, Uta-Renate (R), 92
 "B'nai B'rith and the Challenge of Ethnic Leadership," by Moore, 197
 Boardman, John, and N. G. L. Hammond, editors, "The Cambridge Ancient History," vol. 3, part 3: "The Expansion of the Greek World, Eighth to Sixth Centuries B.C.," 966
 Bobiniska, Celina, and Joseph Goy, editors, "Les Pyrénées et les Carpates, XVI^e–XX^e siècles: Recherches franco-polonaises comparées—Histoire et anthropologie des régions montagneuses et submontagneuses" (E), 233
 Bodnar, John (R), 486
 Bodnar, John, *et al.*, "Lives of Their Own: Blacks, Italians, and Poles in Pittsburgh, 1900–1960," 1089
 Boggs, Carl, "The Impasse of European Communism," 362
 Bogue, Allan G., "The Earnest Men: Republicans of the Civil War Senate," 185
 Böhme, Wolfgang, editor, "Preussen: Eine Herausforderung" (E), 234
 "Böhmen und die böhmischen Stände in der Zeit des beginnenden Zentralismus," by Hassenpflug-Elzholz, 1296
 "Bolivia: The Evolution of a Multi-Ethnic Society," by Klein, 510
 Bolt, Christine (R), 768
 Bolton, S. Charles, "Southern Anglicanism: The Church of England in Colonial South Carolina," 1057
 "Bondebygd i förändring: Bebyggelse och befolkning i västra Närke ca. 1300–1600," by Brunius, 694
 Bonilla, Heraclio, *et al.*, "La independencia en el Perú" (E), 236
 Bonney, Richard, "The King's Debts: Finance and Politics in France, 1589–1661," 123
 Boockmann, Hartmut, *et al.*, editors, "Geschichte und Gegenwart: Festschrift für Karl Dietrich Erdmann" (E), 231
 Boockmann, Hartmut, "Der deutsche Orden: Zwölf Kapitel aus seiner Geschichte," 96
 Booth, P. H. W., "The Financial Administration of the Lordship and County of Chester, 1272–1377," 98
 Borden, Carla M., and Jarrell C. Jackman, editors, "The Muses Flee Hitler: Cultural Transfer and Adaptation, 1930–1945" (E), 1122
 "Borodin: Stalin's Man in China," by Jacobs, 1029
 Borowski, Harry R., "A Hollow Threat: Strategic Air Power and Containment before Korea," 216
 Bosher, J. F. (R), 1272
 "Both Sides of the Ocean: A Biography of Henry Adams," by Chalfant, 1077
 Bottigheimer, Karl S. (R), 992
 Bottigheimer, Karl S., "Ireland and the Irish: A Short

- History," 990
 "The Boundary Hunters: Surveying the 141st Meridian and the Alaska Panhandle," by Green, 1333
 Bourne, Kenneth, "Palmerston: The Early Years, 1784–1841," 677
 Boutilier, James A., editor, "The RCN in Retrospect, 1910–1968" (E), 791
 Bowersock, G. W. (R), 90
 Bowser, Frederick P. (R), 508
 Bowsky, William M. (R), 707
 Boyce, D. George, "Nationalism in Ireland," 121
 Boyd, Carolyn P. (R), 1002
 Boyer, John (C), 249
 Boyer, John W. (R), 434
 Boyer, Paul (R), 1075
 Boyer, Richard (R), 1115
 Boyle, Richard, First Earl of Cork, 992
 Bradley, Keith R. (R), 1253
 Brady, John G., 1328
 Braham, Randolph L. (R), 716
 "Brainpower for the Cold War," by Clowse, 218
 "The Brandeis-Frankfurter Connection," by Murphy, 204
 Brandes, Joseph (R), 215
 Brandt, Harm-Hinrich, "Der österreichische Neoabsolutismus: Staatsfinanzen und Politik, 1848–1860," 134
 Brantley, Cynthia (R), 1039
 Bratzel, John, and Leslie Rout, Jr., *Once Again: Pearl Harbor, Microdots, and J. Edgar Hoover* (Research Note), 953–60
 Braude, Benjamin (R), 1031
 "Brazil and Mexico," edited by Hewlett and Weinert (E), 1125
 Brecht, Bertolt, 1245
 Bregman, Jay, "Synesius of Cyrene: Philosopher-Bishop," 369
 Bremner, Robert H., and Gary W. Reichard, editors, "Reshaping America: Society and Institutions, 1945–1960," 1106
 Brenz, Johannes, 1006
 Breton, Raymond, and Pierre Savard, editors, "The Quebec and Acadian Diaspora in North America" (E), 791
 Breuille, John, "Nationalism and the State," 961
 Bridbury, A. R., "Medieval English Clothmaking: An Economic Survey," 1256
 Briggs, John W. (R), 1088
 Bringham, Newell G., "Saints, Slaves, and Blacks: The Changing Place of Black People within Mormonism," 189
 Brinkley, Alan, "Voices of Protest: Huey Long, Father Coughlin, and the Great Depression," 494
 Britain, Ian, "Fabianism and Culture: A Study in British Socialism and the Arts, c. 1884–1918," 1268
 Brite, J. Duncan (C), 249
 "British and Soviet Communism: The Impact of a Revolution," by Northedge and Wells, 682
 "British Economic and Strategic Planning, 1905–1915," by French, 118
 "British Government in Northern Ireland," by Wallace, 121
 Brockman, Norbert C. (R), 775
 "Broken Promises: How Americans Fail Their Children," by Grubb and Lazerson, 762
 Bronder, Saul E. (R), 1099
 Bronfenbrenner, Martin (R), 1308
 Brook-Shepherd, Gordon, "November 1918," 382
 "Brothers beyond the Sea: National Socialism in Canada," by Wagner, 220
 Brown, Ira V. (R), 472
 Brown, John A., and Robert H. Ruby, "Indians of the Pacific Northwest: A History," 479
 Brown, Judith C., "In the Shadow of Florence: Provincial Society in Renaissance Pescia," 1292
 Brown, Kenneth D., "The English Labour Movement, 1700–1951," 394
 Brown, Richard Harvey (R), 961
 Brown, Sidney Devere (R), 157
 Browning, Reed, "Political and Constitutional Ideas of the Court Whigs," 673
 Bruch, Rüdiger von, "Welpolitik als Kulturmission: Auswärtige Kulturpolitik und Bildungsbürgertum in Deutschland am Vorabend des Ersten Weltkrieges," 1013
 Brucker, Gene, *Tales of Two Cities: Florence and Venice in the Renaissance* (Review Essay), 599–616
 Bruguère, Michel, et al., "Administration et parlement depuis 1815" (E), 1122
 Brundage, Anthony (R), 1265
 Brundage, Burr Cartwright, "The Phoenix of the Western World: Quetzalcoatl and the Sky Religion," 225
 Brundage, James A. (R), 96, 970
 Brune, Lester H., "The Origins of American National Security Policy: Sea Power, Air Power, and Foreign Policy, 1900–1941," 1093
 Bruneau, Thomas C., "The Church in Brazil: The Politics of Religion," 511
 Brunius, Jan, "Bondebygd i förändring: Bebyggelse och befolkning i västra Närke ca. 1300–1600," 694
 Bruns, Roger A., "Knights of the Road: A Hobo History," 494
 Bryson, Thomas A., "Seeds of Mideast Crisis: The United States Diplomatic Role in the Middle East during World War II," 213
 Buck, Lawrence P. (R), 415
 Buckland, Patrick (R), 121
 Buckley, Peter J., and Brian R. Roberts, "European Direct Investment in the U.S.A. before World War I," 490
 Buckley, Roger, "Occupation Diplomacy: Britain, the United States, and Japan, 1945–1952," 1049
 Budd, Ralph, 1086
 BUDDHISM: Harvey, "The Limits to Capital," 1243; Michael, "Rule by Incarnation: Tibetan Buddhism and Its Role in Society and State," 453
 Buder, Stanley (R), 1085
 Buenker, John D. (R), 206
 Buhite, Russell D. (R), 496
 "Building the Escorial," by Kubler, 410
 Bukowski, James B. (R), 432
 Bullard, Melissa M. (R), 1292
 "Bullets and Bureaucrats: The Machine Gun and the United States Army, 1861–1916," by Armstrong, 1094
 Bulliet, Richard W. (R), 439
 Bumke, Joachim, "The Concept of Knighthood in the Middle Ages," 1258
 Burdick, Charles B. (R), 739
 "Die bürgerlichen Fürsorgeanstalten der Reichstadt Frankfurt a.M. im späten Mittelalter," by Moritz, 377
 Burgess, Keith (R), 394
 Burggraaff, Winfield J. (R), 782
 Burk, Kathleen, editor, "War and the State: The Transformation of British Government, 1914–1919" (E), 515
 Burke, Michael E. (R), 506
 Burks, R. V. (R), 140
 Burmeister, Hans Wilhelm, "Prince Philipp Eulenburg-Hertefeld, 1847–1921: His Influence on Kaiser Wilhelm II and His Role in the German

- Government, 1888–1902," 130
 Burner, David (R), 1346
 Burnham, John C. (R), 1065
 Burns, James MacGregor, "The American Experiment: The Vineyard of Liberty," 460
 Burr, Aaron, 1062
 Burt, Larry W., "Tribalism in Crisis: Federal Indian Policy, 1953–1961," 1107
 "Bury St. Edmunds and the Urban Crisis, 1290–1539," by Gottfried, 375
 Busch, Briton C. (R), 1270
 Buse, Dieter K. (R), 704
 Bushman, Richard L. (R), 464
 Bushnell, Amy, "The King's Coffers: Proprietors of the Spanish Florida Treasury, 1565–1702," 223
BUSINESS AND ENTREPRENEURIAL HISTORY: Blackford, "A Portrait Cast in Steel: Buckeye International and Columbus, Ohio," 1085; Bridbury, "Medieval English Clothmaking," 1256; Byrkit, "Forging the Copper Collar: Arizona's Labor-Management War of 1901–1921," 1090; Colburn and Jacoway, editors, "Southern Businessmen and Desegregation," 1343; Collins, "The Business Response to Keynes, 1929–1964," 202; Ferrier, "The History of the British Petroleum Company," vol. 1: "The Developing Years, 1901–1932," 1270; Godley, "The Mandarin-Capitalists from Nanyang," 1043; Harris, "The Right to Manage: Industrial Relations Policies of American Business in the 1940s," 1102; Irick, "Ch'ing Policy toward the Coolie Trade, 1847–1878," 732; Jacob-Wendler, "Deutsche Elektroindustrie in Lateinamerika: Siemens und AEG, 1890–1914," 1352; Johnson, "The Challenge of Change," 1343; Katz *et al.*, "The Social Organization of Early Industrial Capitalism," 1109; Lescure, "Les banques, l'état et le marché immobilier en France à l'époque contemporaine, 1820–1940," 690; Lloyd, "Alien Merchants in England in the High Middle Ages," 661; Meier, "Omkring de fire species: Dansk merkantilistisk stabel- og navigationspolitik i 1720' rne," 1005; Morton, "The Whale's Wake," 1312; Mulholland, "A History of Metals in Colonial America," 1318; O'Brien, "The Nitrate Industry and Chile's Crucial Transition, 1870–1891," 1351; Overton, "Perkins/Budd: Railway Statesmen of the Burlington," 1086; Robbins, "Lumberjacks and Legislators," 1087; Seavoy, "The Origins of the American Business Corporation, 1784–1855," 180; Stivers, "Supremacy and Oil: Iraq, Turkey, and the Anglo-American World Order, 1918–1930," 1270; Thomson, "Clermont-de-Lodève, 1633–1789," 993; Tripathi, "The Dynamics of a Tradition: Kasturbhai Lalbhai and His Entrepreneurship," 1051
 "The Business Response to Keynes, 1929–1964," by Collins, 202
 Bustin, Edouard (R), 1034
 "The Butcher: The Duke of Cumberland and the Suppression of the 45," by Speck, 684
 Butler, Jeffrey E. (R), 152
 Butler, Perry, "Gladstone—Church, State, and Tractarianism: A Study of His Religious Ideas and Attitudes, 1809–1859," 397
 Butt, John, and Ian Donnachie, "Industrial Archaeology in the British Isles," 114
 Buttar, Annemarie, "Rassisch getrennte Schulen im Süden der USA, 1890–1950: Politische, rechtliche, und ökonomische Faktoren," 482
 Button, H. Warren, and Eugene F. Provenzo, Jr., "History of Education and Culture in America," 1055
 Bynum, Caroline Walker, "Jesus as Mother: Studies in the Spirituality of the High Middle Ages," 370
 Byrkit, James W., "Forging the Copper Collar: Arizona's Labor-Management War of 1901–1921," 1090
 Byrne, Frank L. (R), 1070
 Byrnes, James F., 212, 235
 "Byzanz und die Kreuzfahrerstaaten," by Lilie, 95
 "C. C. Slaughter," by Murrah, 1074
 Caldwell, Lynton K. (R), 669
 "The Calhoun Family and Thomas Green Clemson: The Decline of a Southern Patriarchy," by Lander, 1323
 Calhoun, Craig (R), 394
 "The California Column in New Mexico," by Miller, 1068
 "California Conquered," by Harlow, 1068
 Callahan, North (R), 1059
 Calvin, John, 698
 "The Cambridge Ancient History," vol. 3, part 3: "The Expansion of the Greek World," edited by Boardman and Hammond, 966
 "The Cambridge Economic History of India," vol. 1: "C. 1200–c. 1750," edited by Raychaudhuri and Habib, 735
 "The Cambridge History of Africa," vol. 1: "From the Earliest Times to c. 500 B.C.," edited by Clark, 726
 Cameron, James K. (R), 988
 Cammett, John M. (R), 138
 Camp, Roderic A. (R), 780
 Campbell, Hugh G. (R), 1116
 Campbell, R. H., and A. S. Skinner, "Adam Smith," 977
 Campbell, R. H., and Andrew S. Skinner, editors, "The Origins and Nature of the Scottish Enlightenment" (E), 516
 Campbell, Thomas M. (C), 803
 "Canadian Relations with South Africa," by Tennyson, 1112
 Cannadine, David, editor, "Patricians, Power, and Politics in Nineteenth-Century Towns" (E), 1121
 Canny, Nicholas, "The Upstart Earl: A Study of the Social and Mental World of Richard Boyle, First Earl of Cork, 1566–1643," 992
 Cantor, Leonard, editor, "The English Medieval Landscape" (E), 787
 "Capital and Class in Scotland," edited by Dickson (E), 515
CAPITALISM: Harvey, "The Limits to Capital," 1243
 Capp, Bernard (R), 392
 Cardoso, Lawrence A. (R), 226
 Cardoza, Anthony L. (R), 428
 Carey, John A., "Judicial Reform in France before the Revolution of 1789," 125
 Cargill, Jack (R), 86
 "Carl Schurz," by Trefousse, 474
 "Carlos Montezuma and the Changing World of American Indians," by Iverson, 1078
 Carls-Maire, Alice-Catherine, "La Ville Libre de Dantzig en crise ouverte 24.10.1938–1.9.1939: Crise locale et crise européenne," 1022
 Carlsnaes, Walter, "The Concept of Ideology and Political Analysis: A Critical Examination of Its Usage by Marx, Lenin, and Mannheim," 77
 Carlson, Andrew R. (R), 1013
 Carlson, Leonard A. (R), 500
 Caro, Robert A., "The Years of Lyndon Johnson," vol. 1: "The Path to Power," 1346
 Carson, Kit, 479
 Carsten, F. L., "War against War: British and German

- Radical Movements in the First World War," 382
 Carter, Dan T. (R), 772
 Carver, George Washington, 197
 Casdorph, Paul D. (R), 199
 Cassanelli, Lee V., "The Shaping of Somali Society: Reconstructing the History of a Pastoral People, 1600–1900," 1038
 Cassidy, James H. (R), 488
 "Castiglione: The Ideal and the Real in Renaissance Culture," edited by Hanning and Rosand (E), 1122
 Catherine II ("the Great"), 1025
 CATHOLICISM: Alberigo, "Chiesa conciliare," 93; Barstow, "Married Priests and the Reforming Papacy," 660; Blantz, "A Priest in Public Service: Francis J. Haas and the New Deal," 1099; Bruneau, "The Church in Brazil: The Politics of Religion," 511; Dussel, "A History of the Church in Latin America," 778; Fogarty, "The Vatican and the American Hierarchy from 1870 to 1965," 1333; Gueiros Vieira, "O protestantismo, a maçonaria e a questão religiosa no Brasil," 228; Hallenbeck, "Pavia and Rome: The Lombard Monarchy and the Papacy in the Eighth Century," 665; Hamilton, "The Latin Church in the Crusader States," 96; Heinemeyer, "Das Erzbistum Mainz in römischer und fränkischer Zeit," 101; Henningsen, "The Witches' Advocate: Basque Witchcraft and the Spanish Inquisition," 692; Holmes, "Resistance and Compromise," 671; Kauffman, "Faith and Fraternalism: The History of the Knights of Columbus," 775; Lukács, "The Vatican and Hungary, 1846–1878," 1295; McGuire, "The Cistercians in Denmark," 664; Millet, "Les chanoines du chapitre cathédral de Laon, 1272–1412," 1257; Partner, "The Murdered Magicians: The Templars and Their Myth," 970; Patelos, "Vatican I et les évêques uniates," 431; Prodi, "Il sovrano profetice—Un corpo e due anime: La monarchia papale nella prima età moderna," 1016; Semararo, "Restaurazione chiesa e società," 709; Tuck, "The Holy War in Los Altos," 1116; Urban, "The Livonian Crusade," 96; Weinstein and Bell, "Saints and Society: The Two Worlds of Western Christendom, 100–1700," 1255
 "Catholics and Radicals: The Association of Catholic Trade Unionists and the American Labor Movement," by Seaton, 208
 Cavallie, James, "De höga officerarna: Studier i den svenska militära hierarkien under 1600-talets senare del," 695
 Cavarnos, John P. (R), 380
 Cecil, Lamar (R), 700
 Cell, John W., "The Highest Stage of White Supremacy: The Origins of Segregation in South Africa and the American South," 965
 "Censur och propaganda: Svensk informationspolitik under 1900-talets första decennier," by Kilander, 412
 "The Census in British India," edited by Barrier, 737
 "Centrifugal and Centripetal Forces in the Early Dutch Republic: The States of Overijssel, 1566–1600," by Reitsma, 1278
 Chalfant, Edward, "Both Sides of the Ocean: A Biography of Henry Adams, His First Life, 1838–1862," 1077
 Challenger, Richard D. (R), 407
 "The Challenge of Change," by Johnson, 1343
 Chalmers, David (R), 490
 Chambers, Clarke A. (R), 483
 Chan, Albert, "The Glory and Fall of the Ming Dynasty," 448
 Chan, Wellington K. K. (R), 731
 "The Changing Face of Inequality," by Zunz, 1088
 "A Changing Perspective: Attitudes toward Creole Society in New Spain, 1521–1610" by Bacigalupo, 1113
 "Les chanoines du chapitre cathédral de Laon, 1272–1412," by Millet, 1257
 Chao Tz'u-chen, 154
 Chapman, S. D., and S. Chassagne, "European Textile Printers in the Eighteenth Century: A Study of Peel and Oberkampf," 104
 Charanos, Peter (R), 379
 "Charles Gravier, Comte de Vergennes," by Murphy, 996
 Charles, Archduke of Austria, 426
 "The Chartist Experience," edited by Epstein and Thompson (E), 1121
 Chase, Myrna (R), 407
 Chassagne, S., and S. D. Chapman, "European Textile Printers in the Eighteenth Century: A Study of Peel and Oberkampf," 104
 Chauncy, Charles, 749
 Chelkowski, Peter E. (R), 723
 Ch'en, Jerome, "State Economic Policies of the Ch'ing Government, 1840–1895," 731
 Ch'en, Paul Heng-Chao, "The Formation of the Early Meiji Legal Order: The Japanese Code of 1871 and Its Chinese Foundation," 733
 Ch'en Liang, 153
 Ch'en Po-ta, 451
 Cheng, J. Chester (R), 156
 "Chief Left Hand," by Coel, 479
 "Chiesa conciliare," by Alberigo, 93
 "Child Abuse and Moral Reform in England, 1870–1908," by Behlmer, 1268
 Childs, John, "Armies and Warfare in Europe, 1648–1789," 1242
 "Chile in the Nitrate Era: The Evolution of Economic Dependence," by Monteón, 1351
 "Chinese Elites and Political Change: Zhejiang Province in the Early Twentieth Century," by Schoppa, 733
 "Chinese Mine Labour in the Transvaal," by Richardson, 1041
 "Ch'ing Policy toward the Coolie Trade, 1847–1878," by Irick, 732
 Chojnacki, Stanley (R), 1017
 Chrisman, Miriam Usher, "Bibliography of Strasbourg Imprints, 1480–1599," 1005
 Chrisman, Miriam Usher, "Lay Culture, Learned Culture: Books and Social Change in Strasbourg, 1480–1599," 1005
 "Christian Córdoba: The City and Its Region in the Late Middle Ages," by Edwards, 1276
 "Christian Magistrate and State Church: The Reforming Career of Johannes Brenz," by Estes, 1006
 "The Christian Polity of John Calvin," by Höpfl, 698
 "Christianity in Roman Britain to A.D. 500," by Thomas, 967
 Christie, Ian R., "Wars and Revolutions: Britain, 1760–1815," 675
 "Christliche Theologie in China," by Glüer, 154
 Christoff, Peter K., "An Introduction to Nineteenth-Century Russian Slavophilism," vol. 3: "K. S. Aksakov: A Study in Ideas," 1026
 Ch'ü Ch'iu-pai, 155
 Chu Hsi, 153
 "The Church and Healing," edited by Sheils (E), 1355
 "The Church in Brazil: The Politics of Religion," by Bruneau, 511
 Church, Roy (R), 680

- "Churchill and De Gaulle," by Kersaudy, 107
 "The CIA in Guatemala," by Immerman, 781
 Cienciala, Anna M. (R), 1022
 Cinel, Dino, "From Italy to San Francisco: The Immigrant Experience," 1088
 Cipolla, Carlo M., "The Monetary Policy of Fourteenth-Century Florence," 1291
 "The Cistercians in Denmark," by McGuire, 664
 "Citizens in Arms: The Army and the Militia in American Society to the War of 1812," by Cress, 751
 "Città e territorio tra medioevo ed età moderna: Perugia, secc. XIII–XVI," by Grohmann, 707
 "The City and the Saloon," by Noel, 757
 "City Hospitals," by Dowling, 764
 "The City-State in Five Cultures," edited by Griffith and Thomas, 81
 "The Civilian Elite of Cairo in the Later Middle Ages," by Petry, 443
 "The Civilian Population and the Warsaw Uprising of 1944," by Hanson, 1022
 Clapham, Christopher (R), 1305
 Clarfield, Gerard H. (R), 1062
 Clark, J. C. D., "The Dynamics of Change: The Crisis of the 1750s and English Party Systems," 674
 Clark, J. Desmond, editor, "The Cambridge History of Africa," vol. 1: "From the Earliest Times to c. 500 B.C.," 726
 Clark, Manning, 742
 Clark, Manning (R), 1054
 Clark, Margaret (R), 458, 1311
 Clark, Paul F., "The Miners' Fight for Democracy: Arnold Miller and the Reform of the United Mine Workers," 1096
 Clasen, Claus-Peter (R), 700
 "Class, Culture, and the Classroom," by Eagan, 207
 "Class, Race, and Colonialism in West Malaysia," by Stenson, 1311
 Clawson, Robert W., and Lawrence S. Kaplan, editors, "NATO after Thirty Years" (E), 232
 Clawson, Robert W., and Lawrence S. Kaplan, editors, "The Warsaw Pact: Political Purpose and Military Means" (E), 233
 Clements, Barbara Evans (R), 721
 Clements, Kendrick A., editor, "James F. Byrnes and the Origins of the Cold War" (E), 235
 Clemson, Thomas Green, 1323
 "Clermont-de-Lodève, 1633–1789," by Thomson, 993
 Clowse, Barbara Barksdale, "Brainpower for the Cold War: The Sputnik Crisis and the National Defense Education Act of 1958," 218
 Clubb, Jerome M., *et al.*, editors, "Analyzing Electoral History: A Guide to the Study of American Voter Behavior," 168
 Cobb, James C., "The Selling of the South: The Southern Crusade for Industrial Development, 1936–1980," 1101
Cobden as Educator: The Free-Trade Internationalism of Edward Bernstein, 1899–1914, by Fletcher, 561–78
 "The Code of the West," by Rosenberg, 477
 Coel, Margaret, "Chief Left Hand: Southern Arapaho," 479
 Cohen, David, "La promotion des Juifs en France à l'époque du Second Empire, 1852–1870," vol. 1: "Pouvoir et minorité"; vol. 2: "La promotion et l'intégration," 405
 Cohen, Gary B., "The Politics of Ethnic Survival: Germans in Prague, 1861–1914," 434
 Cohen, Jeremy, "The Friars and the Jews: The Evolution of Medieval Anti-Judaism," 93
 Cohen, Naomi W. (R), 198
 Cohen, Shaye J. D., "Josephus in Galilee and Rome: His Vita and Development as a Historian," 89
 Cohen, Sheldon S. (R), 176
 Cohen, Stuart A., "English Zionists and British Jews: The Communal Politics of Anglo-Jewry, 1895–1920," 682
 Cohen, Warren I. (R), 776
 Cohen, William B. (R), 728
 Colburn, David R., and Elizabeth Jacoway, editors, "Southern Businessmen and Desegregation," 1343
 Colburn, David R., and George E. Pozzetta, editors, "Reform and Reformers in the Progressive Era" (E), 1357
 COLD WAR: Borowski, "A Hollow Threat: Strategic Air Power," 216; Clowse, "Brainpower for the Cold War," 218; Kutler, "The American Inquisition," 1104; Martel, "Lend-Lease, Loans, and the Coming of the Cold War," 211; McMahon, "Colonialism and Cold War: The United States and the Struggle for Indonesian Independence," 498; Messer, "The End of an Alliance: James F. Byrnes, Roosevelt, Truman, and the Origins of the Cold War," 212; Theoharis, editor, "Beyond the Hiss Case," 1105
 Coleman, William, "Death Is a Social Disease: Public Health and Political Economy in Early Industrial France," 996
 Coles, Harry L. (R), 216, 1315
 Colley, Linda, "In Defiance of Oligarchy: The Tory Party, 1714–60," 390
 Collier, Ruth Berins, "Regimes in Tropical Africa: Changing Forms of Supremacy, 1945–1975," 1034
 Collingwood, R. G., 73
 Collins, Robert M., "The Business Response to Keynes, 1929–1964," 202
 "Cologne entre Napoléon et Bismarck," by Ayçoberry, 418
 "Colonial Evangelism: A Socio-Historical Study of an East African Mission," by Beidelman, 729
 "Colonial New Hampshire," by Daniell, 172
 "The Colonial Office, War, and Development Policy," by Lee and Petter, 119
 COLONIALISM: Ambrosini, "Paesi e mari ignoti: America e colonialismo europeo nella cultura veneziana, secoli XVI–XVII," 1017; Bade, editor, "Imperialismus und Kolonialmission: Kaiserliches Deutschland und koloniales Imperium," 1011; Baumgart, "Imperialism: The Idea and Reality of British and French Colonial Expansion," 667; Field, "Toward a Programme of Imperial Life: The British Empire," 667; Johnson, "Adjustment to Empire: The New England Colonies, 1675–1715," 462; Jones, "License for Empire: Colonialism by Treaty in Early America," 747; Kamoche, "Imperial Trusteeship and Political Evolution in Kenya, 1923–1963," 151; Kesner, "Economic Control and Colonial Development," 117; Kiernan, "From Conquest to Collapse," 651; Manning, "Slavery, Colonialism, and Economic Growth in Dahomey, 1640–1960," 727; M'Bokolo, "Noirs et Blancs en Afrique en Afrique Équatoriale," 446; Pearce, "The Turning Point in Africa: British Colonial Policy, 1938–48," 445; Sosin, "English America and the Revolution of 1688," 1058; Stern, "Peru's Indian Peoples and the Challenge of Spanish Conquest: Huamanga to 1640," 508; Thorpe, "Ramparts lointains: La politique française des travaux publics à Terre-Neuve et à l'Île Royale, 1695–1758," 219
 "Colonialism and Cold War: The United States and the Struggle for Indonesian Independence, 1945–49," by McMahon, 498
 "The Coming of Austrian Fascism," by Kitchen, 135
 "The Coming of the Mass Market, 1850–1914," by Fraser, 115

- "Comintern Army: The International Brigades and the Spanish Civil War," by Richardson, 693
- "Common Landscape of America, 1580–1845," by Stilgoe, 166
- "Commonwealth to Protectorate," by Woolrych, 672
- COMMUNISM: Boggs, "The Impasse of European Communism," 362; Femia, "Gramsci's Political Thought," 362; Holmberg, "Folk-makt, folkfront, folkdemokrati: De svenska kommunisterna och demokratifrågan, 1943–1977," 413; Isserman, "Which Side Were You On? The American Communist Party during the Second World War," 776; Jacoby, "Dialectic of Defeat," 78; Khanh, "Vietnamese Communism, 1925–1945," 459; Löwy, "The Politics of Combined and Uneven Development," 76; Lyons, "Philadelphia Communists, 1936–1956," 1101; Narkiewicz, "Marxism and the Reality of Power, 1919–1980," 363; Northedge and Wells, "British and Soviet Communism: The Impact of a Revolution," 682; Page, "The Geopolitics of Leninism," 76; Rousset, "Communisme et nationalisme Vietnamien," 1054; Wylie, "The Emergence of Maoism," 451
- "Communisme et nationalisme Vietnamien," by Rousset, 1054
- "Company Shops: The Town Built by a Railroad," by Stokes, 189
- COMPARATIVE HISTORY: Bailly, *The Adjustment of Italian Immigrants in Buenos Aires and New York, 1870–1914*, 281–305; Collier, "Regimes in Tropical Africa: Changing Forms of Supremacy, 1945–1975," 1034; Ellis, *Free and Coerced Transatlantic Migrations*, 251–80; Griffith and Thomas, editors, "The City-State in Five Cultures," 81; Israel, "The Dutch Republic and the Hispanic World, 1606–1661," 1003; Klein, *The Integration of Italian Immigrants into the United States and Argentina* (Forum), 306–29; Lamar and Thompson, editors, "The Frontier in History," 84; Le Roy Ladurie and Goy, "Tithe and Agrarian History from the Fourteenth to the Nineteenth Centuries," 1240; Lewis, "The Muslim Discovery of Europe," 439; Rosenberg and Young, "Transforming Russia and China," 79; Smith, "Conflict and Compromise: Class Formation in English Society, 1830–1914," 982
- "The Complete Anti-Federalist," edited by Storing, 467
- "Compulsory Health Insurance," edited by Numbers (E), 791
- "The Concept of Ideology and Political Analysis," by Carlsnaes, 77
- "The Concept of Knighthood in the Middle Ages," by Bumke, 1258
- "Conceptions of God in Ancient Egypt," by Horning, 1250
- Cone, Carl B. (R), 675
- "The Confederate Navy in Europe," by Spencer, 1325
- "Conflict and Compromise: Class Formation in English Society, 1830–1914," by Smith, 982
- "Conflict in Modern Japanese History," edited by Najita and Koschmann, 1047
- Connelly, Mark T. (R), 1084
- Connolly, S. J., "Priests and People in Pre-Famine Ireland, 1780–1845," 400
- "The Conquest of Morocco," by Porch, 1304
- Conrad, David E. (R), 502
- "Consensus and Disunity: The Lloyd George Coalition Government," by Morgan, 118
- CONSERVATION AND THE ENVIRONMENT: Allin, "The Politics of Wilderness Preservation," 759; Lockmann, "Guarding the Forests of Southern California," 202; Pyne, "Fire in America: A Cultural History of Wildland and Rural Fire," 759; Righter, "Crucible for Conservation: The Creation of Grand Teton National Park," 759
- CONSERVATISM: Nygård, "Suomalainen äärioikeisto maailmansotien välillä," 1280
- Constable, Giles, and Alexander Kazhdan, "People and Power in Byzantium: An Introduction to Modern Byzantine Studies," 379
- Constantine, 660
- CONSTITUTIONAL HISTORY: Grünthal, "Parlamentarismus in Preussen, 1848/49–1857/58," 1008; Hoffman, "Governmental Secrecy and the Founding Fathers," 179; Hyman and Wiecek, "Equal Justice under Law: Constitutional Development, 1835–1875," 469; Keynes, "Undeclared War: Twilight Zone of Constitutional Power," 1056
- "Consul of God," by Richards, 92
- "Continuity and Change in Austrian Socialism," by Sully, 1290
- "Continuity and Change in Marxism," edited by Fischer *et al.* (E), 1356
- Contours of Slavery and Social Change in Africa*, by Manning, 835–57
- "Contrary Commonwealth: The Theme of Exile in Medieval and Renaissance Italy," by Starn, 1258
- "Controversial Ideas in China and in Europe," by Witek, 1042
- Cook, Noble David, "Demographic Collapse: Indian Peru, 1520–1620," 509
- Cook, Noble David, "The People of the Colca Valley: A Population Study," 783
- Cooke, James J. (R), 1304
- Cookson, J. E., "The Friends of Peace: Anti-War Liberalism in England, 1793–1815," 111
- Cooley, James C., Jr., "T. F. Wade in China: Pioneer in Global Diplomacy, 1842–1882," 1043
- Cooper, Samuel, 1320
- Copenhagen, Brian P. (R), 647
- Coppa, Frank J. (R), 1293
- Corbin, David Alan, "Life, Work, and Rebellion in the Coal Fields: The Southern West Virginia Miners, 1880–1922," 1096
- Corfield, P. J., "The Impact of English Towns, 1700–1800," 676
- Cork, Richard Boyle, First Earl of, 992
- Cornebise, Alfred E., "Typhus and Doughboys: The American Polish Typhus Relief Expedition, 1919–1921," 1335
- Cornell, Lasse (R), 695
- Cornell, Lasse, "Sundsvallsdistriktets sågverksarbetare, 1860–1890: Arbete, levnadsförhållanden, rekrytering," 412
- Corni, Gustavo, "Stato assoluto e società agraria in Prussia nell'età di Federico II," 1285
- "Corporate Liberalism: The Origins of Modern American Political Theory, 1890–1920," by Lustig, 1083
- "Corporatism and Consensus in Florentine Electoral Politics, 1280–1400," by Najemy, 1259
- Cortner, Richard C. (R), 205
- Cott, Nancy F. (R), 173
- Cotter, John L. (R), 170
- "Cotton Fields and Skyscrapers: Southern City and Region, 1607–1980," by Goldfield, 1314
- Cottrol, Robert J., "The Afro-Yankees: Providence's Black Community in the Antebellum Era," 1324
- Coughlin, Charles Edward, 494
- Coulson, Andrew, "Tanzania: A Political Economy," 1040
- "The Counter-Reformation," by Wright, 698
- "Counting Sheep: From Open Range to Agribusiness

- on the Columbia Plateau." by McGregor, 1087
- "Court and Bakufu in Japan: Essays in Kamakura History," edited by Mass (E), 1124
- Courtwright, David T., "Dark Paradise: Opiate Addiction in America before 1940," 169
- Cowan, Ian B., "The Scottish Reformation: Church and Society in Sixteenth-Century Scotland," 988
- "The Cowboy Hero," by Savage, 756
- Cowdrey, H. E. J. (R), 99
- Cowen, David L. (R), 169
- Cowing, Cedric B. (R), 1320
- Cox, Jeffrey, "The English Churches in a Secular Society: Lambeth, 1870–1930," 983
- Cox, Lawanda, "Lincoln and Black Freedom: A Study in Presidential Leadership," 473
- Cox, Thomas R. (R), 202
- Craddock, Patricia B., "Young Edward Gibbon: Gentleman of Letters," 110
- "Cradle of the Middle Class: The Family in Oneida County, New York," by Ryan, 752
- Craig, Gordon A. (R), 1239
- Craig, Gordon A., "The Germans," 1281
- Craig, Gordon A., *The Historian and the Study of International Relations*, 1–11
- Craig, Gordon A., and Alexander L. George, "Force and Statecraft: Diplomatic Problems of Our Time," 1246
- Cranz, Galen, "The Politics of Park Design: A History of Urban Parks in America," 1334
- Craven, Paul, "'An Impartial Umpire': Industrial Relations and the Canadian State, 1900–1911," 219
- "Creation of a Mexican Landscape: Territorial Organization and Settlement in the Eastern Puebla Basin," by Licate, 779
- Crecelius, Daniel (R), 443
- Crecelius, Daniel, "The Roots of Modern Egypt: A Study of the Regimes of 'Ali Bey al-Kabir and Muhammed Bey Abu al-Dhahab, 1760–1775," 443
- Cress, Lawrence Delbert, "Citizens in Arms: The Army and the Militia in American Society to the War of 1812," 751
- CRIME, CRIMINALS, AND THE POLICE: Jones, "Crime, Protest, Community, and Police in Nineteenth-Century Britain," 115; Monkkonen, "Police in Urban America, 1860–1920," 190; O'Brien, "The Promise of Punishment: Prisons in Nineteenth-Century France," 689; Van der Stroom, "Duitse strafrechtspleging in Nederland en het lot der veroordeelden," 1004
- "Crime, Protest, Community, and Police in Nineteenth-Century Britain," by Jones, 115
- Crimmins, Timothy J. (R), 1314
- Criscenti, Joseph T. (R), 1119
- "Crises in the World-System," edited by Bergesen (E), 1355
- "Crisis and Conflict: World News Reporting between Two Wars," by Desmond, 1248
- "The Crisis of the Old Order in Russia," by Manning, 1028
- "The Critical Historians of Art," by Podro, 1244
- Croizier, Ralph (R), 451
- Cronc, Patricia (R), 440
- Gronin, James E., and Jonathan Schneer, editors, "Social Conflict and the Political Order in Modern Britain" (E), 788
- Crook, J. Mordaunt (R), 738
- Crosby, Alfred W. (R), 1335
- Cross, Harry E., and James A. Sandos, "Across the Border: Rural Development in Mexico and Recent Migration to the United States," 226
- Cross, J. A., "Lord Swinton," 1271
- Cross, Michael S. (R), 1349
- Crouch, Dora P. (R), 1334
- Crouch, Dora P., *et al.*, "Spanish City Planning in North America," 166
- "Crucible for Conservation: The Creation of Grand Teton National Park," by Righter, 759
- Cruikshanks, Eveline, editor, "Ideology and Conspiracy: Aspects of Jacobitism, 1689–1759" (E), 788
- Crunden, Robert M., "Ministers of Reform: The Progressives' Achievement in American Civilization, 1889–1920," 762
- "Crusaders for Fitness," by Whorton, 1066
- Culbert, David (R), 765
- "The Cult of the Revolutionary Tradition," by Hutton, 126
- "La cultura italiana tra '800 e '900 e le origini del nazionalismo" (E), 235
- CULTURAL HISTORY: Banks, "Victorian Values: Secularism and the Size of Families," 395; Bruch, "Weltpolitik als Kulturmission: Auswärtige Kulturpolitik und Bildungsbürgertum in Deutschland am Vorabend des Ersten Weltkrieges," 1013; Bumke, "The Concept of Knighthood in the Middle Ages," 1258; Button and Provenzo, "History of Education and Culture in America," 1055; Chrisman, "Lay Culture, Learned Culture: Books and Social Change in Strasbourg, 1480–1599," 1005; Gurevich, "Problemy srednevekovoi narodnoi kul'tury," 666; Hall, "The Organization of American Culture, 1700–1900," 165; Hatch and Noll, editors, "The Bible in America," 744; Isaac, "The Transformation of Virginia, 1740–1790," 464; Lehmann, "The Roots of Modern Japan," 1047; Lombardi, "Venezuela," 782; Marling, "Wall-to-Wall America: A Cultural History of Post-Office Murals in the Great Depression," 775; Sackett, "Popular Entertainment, Class, and Politics in Munich, 1900–1923," 1285; Savage, "The Cowboy Hero," 756; Semenova, "Ocherki istorii byta i kul'turnoi zhizni Rossii," 1024; Shedel, "Art and Society: The New Art Movement in Vienna, 1897–1914," 426; Stark and Lackner, editors, "Essays on Culture and Society in Modern Germany," 1282; Trachtenberg, "The Incorporation of America: Culture and Society in the Gilded Age," 193; Turner, "Beware the People Weeping: Public Opinion and the Assassination of Abraham Lincoln," 1070; Wagenknecht, "American Profile, 1900–1909," 490; Weber, "The Mexican Frontier, 1821–1826," 504; Willett, "Art and Politics in the Weimar Period," 420
- "Culture in Comparative and Evolutionary Perspective," by Leopold, 649
- Cumberland, William Augustus, Duke of, 684
- Cummings, Bruce, "The Origins of the Korean War: Liberation and the Emergence of Separate Regimes, 1945–1947," 159
- Cunliffe-Lister, Philip, Lord Swinton, 1271
- Cunningham, Hugh (C), 803
- Cunningham, Hugh (R), 109
- Cunningham, Noble E., Jr. (R), 468
- Curl, Donald W. (R), 1082
- Curran, Joseph M. (R), 1272
- Curry, Leonard P. (R), 1324
- Curry, Richard O. (R), 185
- Curtin, Patricia Romero, *Laboratory for the Oral History of Slavery: The Island of Lamu on the Kenya Coast*, 858–82
- Curtis, Michael (R), 652
- Cusanus, Nicolaus, 970
- Cushman, Nicholas P. (R), 783
- Cushman, Nicholas P., "Farm and Factory: The Jesuits

- and the Development of Agrarian Capitalism in Colonial Quito, 1600–1767," 1350
- "Custom, Work, and Market Capitalism: The Forest of Dean Colliers, 1788–1888," by Fisher, 394
- Cuttler, S. H., "The Law of Treason and Treason Trials in Later Medieval France," 100
- "Czechoslovak Policy and the Hungarian Minority, 1945–1948," by Janics, 714
- Czitrom, Daniel J., "Media and the American Mind: From Morse to McLuhan," 745
- Dahlstrand, Frederick C., "Amos Bronson Alcott: An Intellectual Biography," 761
- Dahrendorf, Ralf, "On Britain," 1261
- Dale, Stephen F. (R), 737
- Dales, Richard C. (R), 663
- "Damned Indians: The Pick-Sloan Plan and the Missouri River Sioux, 1944–1980," by Lawson, 500
- Daniel, Elton L., "The Political and Social History of Khurasan under Abbasid Rule, 747–820," 149
- Daniell, Jere R. (R), 1063
- Daniell, Jere R., "Colonial New Hampshire: A History," 172
- Daniels, Robert V. (C), 1137
- "Dark Paradise: Opiate Addiction in America before 1940," by Courtwright, 169
- Darnton, Robert, "The Literary Underground of the Old Regime," 687
- Davies, Norman, "God's Playground: A History of Poland," vol. 1: "The Origins to 1795"; vol. 2: "1795 to the Present," 436
- Davis, Lance E. (R), 490
- Davis, R. W. (R), 113
- Davison, Roderic H. (R), 1032
- "Davitt and Irish Revolution, 1846–82," by Moody, 401
- Davitt, Michael, 401
- Dawn, C. Ernest (R), 1030
- Dawson, Joseph G., III, "Army Generals and Reconstruction: Louisiana, 1862–1877," 186
- Dean, David M., "Defender of the Race: James Theodore Holly, Black Nationalist Bishop," 1073
- Dean, Warren (R), 782
- "Dear, Dirty Dublin: A City in Distress, 1899–1916," by O'Brien, 685
- "Death and the Enlightenment," by McManners, 124
- "Death Is a Social Disease," by Coleman, 996
- Debenedetti, Charles (R), 206
- De Bruin, R., "Islam en nationalisme in door Japan bezet Indonesië, 1942–1945," 1312
- Debs, Eugene V., 1336
- "Decades of Discontent," edited by Scharf and Jensen (E), 1358
- "The Declaration of Rights, 1689," by Schwoerer, 109
- "Deer Forests, Landlords, and Crofters," by Orr, 990
- "Defender of the Race: James Theodore Holly, Black Nationalist Bishop," by Dean, 1073
- De Gaulle, Charles, 107, 692
- Degler, Carl N. (R), 743
- De Gordon, Bernard, 663
- De Grand, Alexander, "Italian Fascism: Its Origins and Development," 1019
- De Jesus, Ed. C., and Alfred W. McCoy, editors, "Philippine Social History: Global Trade and Local Transformations," 1310
- Delzell, Charles F. (R), 138, 429
- De Madariaga, Isabel, "Russia in the Age of Catherine the Great," 146
- Demaitre, Luke E., "Doctor Bernard de Gordon: Professor and Practitioner," 663
- Demand, Nancy H., "Thebes in the Fifth Century: Heracles Resurgent," 1251
- De Marco, William M., "Ethnics and Enclaves: Boston's Italian North End," 191
- Deme, Laszlo (R), 433
- D'Emilio, John, "Sexual Politics, Sexual Communities: The Making of a Homosexual Minority in the United States, 1940–1970," 1341
- "Democracy and Sectarianism: A Political and Social History of Liverpool, 1868–1939," by Waller, 398
- "Demographic Collapse: Indian Peru, 1520–1620," by Cook, 509
- DEMOGRAPHY: Barrier, editor "The Census in British India," 737; Brunius, "Bondebygd i förändring: Bebyggelse och befolkning i västra Närke ca. 1300–1600," 694; Cook, "Demographic Collapse: Indian Peru, 1520–1620," 509; Eltis, *Free and Coerced Transatlantic Migrations*, 251–80; Friginals, Klein, and Engerman, *The Level and Structure of Slave Prices on Cuban Plantations in the Mid-Nineteenth Century: Some Comparative Perspectives*, 1201–18; Grohmann, "Città e territorio tra medioevo ed età moderna: Perugia, secc. XIII–XVI," 707; Jones, "Village and Seaport: Migration and Society in Eighteenth-Century Massachusetts," 174; Lewandowski, "Migration and Ethnicity in Urban India," 456; McDonald and Muldowny, "TVA and the Dispossessed: The Resettlement of Population in the Norris Dam Area," 1341; Soloway, "Birth Control and the Population Question in England, 1877–1930," 680; Wells, "Revolutions in Americans' Lives: A Demographic Perspective," 743; Wrigley and Schofield, "The Population History of England, 1541–1871," 386
- Demos, John Putnam, "Entertaining Satan: Witchcraft and the Culture of Early New England," 1316
- Dennison, George M. (R), 179
- De Santis, Vincent P. (R), 1075
- "Descent from Glory: Four Generations of the John Adams Family," by Nagel, 1075
- De Silva, K. M., "A History of Sri Lanka," 163
- Desmond, Robert W., "Crisis and Conflict: World News Reporting between Two Wars, 1920–1940," 1248
- Dethloff, Henry C., and Irvin M. May, Jr., editors, "Southwestern Agriculture: Pre-Columbian to Modern," 758
- "Die deutsche Beraterschaft in China," edited by Martin (E), 234
- "Deutsche Elektroindustrie in Lateinamerika: Siemens und AEG, 1890–1914," by Jacob-Wendler, 1352
- "Der deutsche Orden," by Boockmann, 96
- "Deutschland und die Neutralität Italiens, 1914–1915," by Monticone, 1014
- "Deutschland und die ungarische Räterepublik," by Tokody, 1295
- "Deutschland und Frankreich, 1936–1939," edited by Hildebrand and Werner (E), 788
- De Vallone, Yves, 699
- De Vries, Jan (R), 1240
- Dewey, Horace W. (R), 145, 1260
- Dewindt, Anne Reiber, and Edwin Brezette Dewindt, "Royal Justice and the Medieval English Countryside: The Huntingdonshire Eyre of 1286, the Ramsey Abbey Banlieu Court of 1287, and the Assizes of 1287–88," 373
- "Dialectic of Defeat," by Jacoby, 78
- "The Diary of Beatrice Webb," vol. 1: "Glitter Around and Darkness Within," 1873–1892," edited by MacKenzie and MacKenzie, 984
- "Diary of John Quincy Adams," edited by Taylor *et al.*, 179

- Dick, Steven J., "Plurality of Worlds: The Origins of the Extraterrestrial Life Debate from Democritus to Kant," 647
- Dickinson, H. T. (R), 977
- Dickinson, John, 1321
- Dickson, Tony, editor, "Capital and Class in Scotland" (E), 515
- Diephouse, David J. (R), 1010
- "The Dilemma of Amazonian Development," edited by Moran (E), 1358
- Dinnerstein, Leonard, "America and the Survivors of the Holocaust," 215
- Diocletian, Jovius, 660
- DIPLOMATIC HISTORY: Appadorai, "The Domestic Roots of India's Foreign Policy, 1947-1972," 162; Artaud, "La question des dettes interalliées et la reconstruction de l'Europe (1917-1929)," 106; Buckley, "Occupation Diplomacy: Britain, the United States, and Japan, 1945-1952," 1049; Cooley, "T. F. Wade in China," 1043; Craig and George, "Force and Statecraft: Diplomatic Problems of Our Time," 1246; Fox, "Germany and the Far Eastern Crisis," 421; Frucht, "Dunarea Noastra: Romania, the Great Powers, and the Danube Question, 1914-1921," 712; Gajda, "Postscript to Victory," 120; Gravson, "Saudi-American Relations," 499; Holloway, "Liberian Diplomacy in Africa," 1036; Jacobs, "America and the Winter War, 1939-1940," 210; Kersaudy, "Churchill and De Gaulle," 107; Kuropiatnik, "Rossiia i SShA: Ekonomicheskii, kul'turnye i diplomaticheskie sviazi, 1867-1881," 1300; Lukas, "Bitter Legacy: Polish-American Relations in the Wake of World War II," 1103; Murphy, "Charles Gravier, Comte de Vergennes," 996; Nouailhat, "France et États-Unis, août 1914-avril 1917," 766; Olliff, "Reforma Mexico and the United States," 1067; Rabe, "The Road to OPEC: United States Relations with Venezuela, 1919-1976," 499; Renzi, *Who Composed Sazonov's Thirteen Points? A Re-Examination of Russia's War Aims of 1914* (Research Note), 347-57; Roeder, "Austria's Eastern Question, 1700-1790," 711; Schober, "Die Tiroler Frage auf der Friedenskonferenz von Saint Germain," 1290; Schulte Nordholt, "The Dutch Republic and American Independence," 466; Tennyson, "Canadian Relations with South Africa," 1112; Tokody, "Deutschland und die ungarische Räterepublik," 1295; Wittner, "American Intervention in Greece, 1943-1949," 213
- Dippie, Brian W. (R), 479
- Dippie, Brian W., "The Vanishing American: White Attitudes and U.S. Indian Policy," 744
- Di Scala, Spencer M. (R), 427, 963
- "The Discovery of Insulin," by Bliss, 964
- Disraeli, Benjamin, 981
- "Disraeli's Grand Tour," by Blake, 981
- "The Divine Community: Trinity, Church, and Ethics in Reformation Theologies," by Loeschen, 380
- "The Divine Politician: Samuel Cooper and the American Revolution in Boston," by Akers, 1320
- "Divorce in New Zealand," by Phillips, 164
- Dobbs, Charles M. (C), 527
- Dobkowski, Michael N. (R), 197
- Dockrill, M. L. (R), 120
- Dockes, Pierre, "Medieval Slavery and Liberation," 372
- "Doctor Bernard de Gordon," by Demaitre, 663
- Dodd, Philip, editor, "The Art of Travel: Essays on Travel Writing" (E), 1120
- Dodgshon, Robert A., "Land and Society in Early Scotland," 376
- Doerries, Reinhard R. (R), 1014
- "The Domestic Roots of India's Foreign Policy, 1947-1972," 162
- "The Dominican Republic," by Wiarda and Kryzanek, 503
- Dominick, Raymond H., III, "Wilhelm Liebknecht and the Founding of the German Social Democratic Party," 1012
- Donnachie, Ian (R), 1264
- Donnachie, Ian, and John Butt, "Industrial Archaeology in the British Isles," 114
- Donner, Fred McGraw, "The Early Islamic Conquests," 440
- Dorn, Jacob H. (R), 192
- Dorsch, Volker, "Die Handelskammern der Rheinprovinz in der zweiten Hälfte des 19. Jahrhunderts: Eine Studie zur Funktion und Entwicklung wirtschaftlicher Interessenvertretungen," 1009
- Douglas, James, Earl of Morton, 683
- Douglas, Roy, "New Alliances, 1940-41," 364
- Douglas, W. A. B. (R), 987
- Douglass, Elisha P. (R), 180
- Dowling, Harry F., "City Hospitals: The Undercare of the Underprivileged," 764
- Drago, Edmund L., "Black Politicians and Reconstruction in Georgia: A Splendid Failure," 1071
- Drake, Paul W. (R), 224
- "Dreaming of What Might Be: Knights of Labor in Ontario, 1880-1900," by Kealey and Palmer, 1349
- Drew, K. F. (R), 91
- Drews, Robert, "Basileus: The Evidence for Kingship in Geometric Greece," 1251
- Dreyer, Edward L., "Early Ming China: A Political History, 1355-1535," 449
- Dreyer, Frederick, *Faith and Experience in the Thought of John Wesley*, 12-30
- Dreyfus, Alfred, 691
- Dreyfus, Hubert L., and Paul Rabinow, "Michel Foucault: Beyond Structuralism and Hermeneutics," 648
- "Drugs in America," by Morgan, 169
- Drummond, Gordon D., "The German Social Democrats in Opposition, 1949-1960: The Case against Rearmament," 1016
- Druzhinina, E. I., "Iuzhnaia Ukraina v period krizisa feodalizma, 1825-1860 gg.," 718
- Dubofsky, Melvyn (R), 1336
- Ducker, James H. (R), 200, 1328
- Duffy, John (R), 763, 964
- Duffy, John J., and H. Nicholas Muller III, "An Anxious Democracy: Aspects of the 1830s," 1063
- Duggan, Lawrence G. (R), 101
- Dugger, Ronnie, "The Politician: The Life and Times of Lyndon Johnson, the Drive for Power—From the Frontier to Master of the Senate," 502
- Duiker, William J. (R), 1054
- "Duitse strafrechtspleging in Nederland en het lot der veroordeelden," by Van der Stroom, 1004
- Dull, Jack L. (R), 152
- Dull, Jonathan R. (R), 179
- Dulles, John Foster, 217
- Dumin, Frederick (R), 1290
- Dumke, Rolf Horst (R), 1009
- "Dunarea Noastra: Romania, the Great Powers, and the Danube Question, 1914-1921," by Frucht, 712
- Dunn, D. Elwood (R), 1036
- Dunn, John (R), 392
- Dunne, Gerald T. (R), 496
- Duram, James C., "A Moderate among Extremists: Dwight D. Eisenhower and the School

- Desegregation Crisis," 1345
 Durden, Robert F. (R), 1083
 Durey, Michael J. (R), 676
 Dussel, Enrique, "A History of the Church in Latin America: Colonialism to Liberation, 1492–1979," 778
 "The Dutch Republic and American Independence," by Schulte Nordholt, 466
 "The Dutch Republic and the Hispanic World, 1606–1661," by Israel, 1003
 Du Toit, André, *No Chosen People: The Myth of the Calvinist Origins of Afrikaner Nationalism and Racial Ideology*, 920–52
 "Dvoevlastie v Sibiri," by Babikova, 719
 "Dwight D. Eisenhower," by Lee, 215
 Dwyer, John, *et al.*, editors, "New Perspectives on the Politics and Culture of Early Modern Scotland" (E), 788
 Dyer, Alan (R), 676
 Dykstra, Robert R. (R), 185
 "The Dynamics of Change: The Crisis of the 1750s and English Party Systems," by Clark, 674
 "The Dynamics of a Tradition: Kasturbhai Lalbhai and His Entrepreneurship," by Tripathi, 1051
 Dyson, Lowell K., "Red Harvest: The Communist Party and American Farmers," 208
- Eagan, Eileen (C), 1141
 Eagan, Eileen, "Class, Culture, and the Classroom: The Student Peace Movement of the 1930s," 207
 "Earl Warren," by White, 777
 Earle, Carville (R), 171, 1318
 "The Early Islamic Conquests," by Donner, 440
 "Early Maryland in a Wider World," edited by Quinn, 748
 "Early Ming China," by Dreyer, 449
 "The Earnest Men: Republicans of the Civil War Senate," by Bogue, 185
 Eccles, Audrey, "Obstetrics and Gynecology in Tudor and Stuart England," 387
 "L'economia italiana, 1861–1940," edited by Toniolo, 1293
 "Economic Control and Colonial Development," by Kesner, 117
- ECONOMIC HISTORY: Artola, "La hacienda del Antiguo Régimen," 1000; Buckley and Roberts, "European Direct Investment in the U.S.A. before World War I," 490; Bushnell, "The King's Coffin: Proprietors of the Spanish Florida Treasury," 223; Campbell and Skinner, "Adam Smith," 977; Ch'en, "State Economic Policies of the Ch'ing Government, 1840–1895," 731; Cipolla, "The Monetary Policy of Fourteenth-Century Florence," 1291; Coulson, "Tanzania: A Political Economy," 1040; Dahrendorf, "On Britain," 1261; Dorsch, "Die Handelskammern der Rheinprovinz in der zweiten Hälfte des 19. Jahrhunderts," 1009; Ekelund and Tollison, "Mercantilism as a Rent-Seeking Society," 81; Friginals, Klein, and Engerman, *The Level and Structure of Slave Prices on Cuban Plantations in the Mid-Nineteenth Century: Some Comparative Perspectives*, 1201–18; Fraser, "The Coming of the Mass Market, 1850–1914," 115; French, "British Economic and Strategic Planning, 1905–1915," 118; Galenson, "White Servitude in Colonial America," 171; Giorgos, "To zētēma ton trapezōn, 1871–1873," 431; Jacobson, *Is There a New International History of the 1920s?* (Review Essay), 617–45; Jennings and Trout, "The Tontine from Louis XIV to the French Revolutionary Era," 1272; Lampe, "Balkan Economic History, 1550–1950," 139; Leff, "Underdevelopment and Development in Brazil," 782; Levi, "For Want of Trade: Shipping and the New Jersey Ports, 1680–1783," 463; Manning, *Contours of Slavery and Social Change in Africa*, 835–57; May, "From New Deal to New Economics: The American Liberal Response to the Recession of 1937," 495; McGowan, "Economic Life in Ottoman Europe," 430; McKendrick *et al.*, "The Birth of a Consumer Society," 977; Miño Grijalva *et al.*, "Tres aspectos de la presencia española en México," 226; Monteón, "Chile in the Nitrate Era: The Evolution of Economic Dependence," 1351; Nakumura, "The Postwar Japanese Economy," 1308; North, "Structure and Change in Economic History," 360; Rawley, "The Transatlantic Slave Trade," 361; Raychaudhuri and Habib, editors, "The Cambridge Economic History of India," vol. 1: "C. 1200–c. 1750," 735; Silverman, "Reconstructing Europe after the Great War," 383; Toniolo, editor, "L'economia italiana, 1861–1940," 1293; Van Cauwenberghe, "Het vorstelijk domein en de overheidsfinanciën in de Nederlanden, 15de en 16de eeuw," 1277; Walker, "Spanish Politics and Imperial Trade, 1700–1789," 223; Wallerstein, "The Modern World-System," vol. 2: "Mercantilism," 81; Wickins, "An Economic History of Africa from the Earliest Times to Partition," 1034
 "An Economic History of Africa from the Earliest Times to Partition," by Wickins, 1034
 "Economic Life in Ottoman Europe," by McGowan, 430
 Eddy, Samuel (R), 368
 Edelman, Bob (R), 1029
 Edelstein, Alan, "An Unacknowledged Harmony: Philo-Semitism and the Survival of European Jewry," 972
 Edmondson, C. Earl (R), 135
- EDUCATION: Baillargeon, "Le Séminaire de Québec de 1760 à 1800," 1348; Button and Provenzo, "History of Education and Culture in America," 1055; Herbst, "From Crisis to Crisis: American College Government, 1639–1819," 176; Ivanova, "Formirovanie sovetsskoi nauchnoi intelligentsii, 1917–1927 gg.," 720; Jarausch, "Students, Society, and Politics in Imperial Germany," 702; Krumbholz, "Die Elementarbildung in Russland bis zum Jahre 1864," 1027; Lui, "The Hanlin Academy," 1041; McDowell and Webb, "Trinity College, Dublin, 1592–1952," 684; Menk, "Die Hohe Schule Herborn in ihrer Frühzeit, 1584–1660," 1007; Meskill, "Academies in Ming China," 450; Mints and Nenarokov, editors, "Zhenshchiny—Revolutionary i uchenye," 721; Prather, "Resurgent Politics and Educational Progressivism in the New South," 1083; Soltow and Stevens, "The Rise of Literacy and the Common School in the United States," 481; Struminger, "What Were Little Girls and Boys Made of? Primary Education in Rural France, 1830–1880," 1273; Tobias, "Old Dartmouth on Trial," 760; Unger, "Education under Mao," 452; Vázquez *et al.*, "Ensayos sobre historia de la educación en México," 506
 "The Education of Mrs. Henry Adams," by Kaledin, 196
 "Education under Mao," by Unger, 452
 "Edward Jenner: The Cheltenham Years," by Saunders, 676
 Edward VI, 670
 "Edwardian England," edited by Read (E), 1121
 Edwards, Griffith, and Virginia Berridge, "Opium

- and the People: Opiate Use in Nineteenth-Century England," 396
- Edwards, Jerome E., "Pat McCarran: Political Boss of Nevada," 1099
- Edwards, John, "Christian Córdoba: The City and Its Region in the Late Middle Ages," 1276
- Egan, Clifford (R), 470
- Egan, Keith J. (R), 967
- Egerton, George (R), 220
- Ehret, Christopher, and Merrick Posnansky, editors, "The Archaeological and Linguistic Reconstruction of African History" (E), 1357
- Eichholtz, Dietrich, and Kurt Gossweiler, editors, "Faschismus Forschung: Positionen, Probleme, Polemik," 668
- Einstein, Albert, 514
- Eisenhower, Dwight David, 215, 1345
- Ekechi, F. K. (R), 729
- Ekelund, Robert B., Jr., and Robert D. Tollison, "Mercantilism as a Rent-Seeking Society: Economic Regulation in Historical Perspective," 81
- Ekirch, A. Roger, "'Poor Carolina': Politics and Society in Colonial North Carolina, 1729-1776," 463
- Eksteins, Modris (R), 1248
- "Electoral Behavior in Unreformed England," by Phillips, 978
- "Electrons and Valence," by Stranges, 964
- "Die Elementarbildung in Russland bis zum Jahre 1864," by Krumbholz, 1027
- Eley, Geoff (R), 701
- Eliot, T. S., 1245
- ELITES: Barker, "Army, Aristocracy, Monarchy: Essays on War, Society, and Government in Austria, 1618-1780," 425; Hall, "The Organization of American Culture, 1700-1900," 165; Hassenpflug-Elzholz, "Böhmen und die böhmischen Stände in der Zeit des beginnenden Zentralismus," 1296; Hull, "The Entourage of Kaiser Wilhelm II, 1888-1918," 700; Jaher, "The Urban Establishment," 167; Jansson, "Samhällsförändring och sammanslutningsformer," 1279; Liehr, "Sozialgeschichte spanischer Adelskorporationen: Die Maestranzas de Caballería, 1670-1808," 1001; Manning, "The Crisis of the Old Order in Russia," 1028; Meehan-Waters, "Autocracy and Aristocracy: The Russian Service Elite of 1730," 437; Peck, "Northampton," 1262; Petry, "The Civilian Elite of Cairo in the Later Middle Ages," 443; Schoppa, "Chinese Elites and Political Change: Zhejiang Province in the Early Twentieth Century," 733; Solov'ev, "Samoderzhavie i dvorianstvo v 1902-1907 gg.," 1301; Solter, "Adel og embede: Embedsfordeling og karrieremobilitet hos den dansk-norske adel, 1588-1660," 1279
- Elkana, Yehuda, and Gerald Holton, editors, "Albert Einstein: Historical and Cultural Perspectives" (E), 514
- "Ellen Wilkinson," by Vernon, 120
- Eller, Ronald D. (R), 1101
- Eller, Ronald D., "Miners, Millhands, and Mountaineers: Industrialization of the Appalachian South, 1880-1930," 771
- Ellersieck, Heinz, E. (R), 695, 1279
- Ellis, David Maldwyn (R), 1061
- Eltis, David, *Free and Coerced Transatlantic Migrations: Some Comparisons*, 251-80
- "Emancipation in Late Medieval Florence," by Kuehn, 378
- "The Emergence of Maoism," by Wylie, 451
- "The Emergence of the British Two-Party System, 1760-1832," by O'Gorman, 675
- Emerson, Ralph Waldo, 182
- "Emigrazione, diplomazia et cannoniere: L'intervento italiano in Venezuela," by Vernassa, 227
- "Emil Nolde," by Pois, 419
- "Emperors and Lawyers," by Honoré, 658
- "An Empire for the Masses: The French Popular Image of Africa, 1870-1900," by Schneider, 1274
- Enayat, Hamid, "Modern Islamic Political Thought," 724
- "The End of an Alliance: James F. Byrnes, Roosevelt, Truman, and the Origins of the Cold War," by Messer, 212
- "Das Ende Preussens in französischer Sicht," by Köhler, 1015
- "The Endless War: Fifty Years of Struggle in Vietnam," by Harrison, 740
- Engerman, Stanley L., Manuel Moreno Fraginals, and Herbert S. Klein, *The Level and Structure of Slave Prices on Cuban Plantations in the Mid-Nineteenth Century: Some Comparative Perspectives*, 1201-18
- "England's Rise to Greatness, 1668-1763," edited by Baxter (E), 1121
- Englisch, Norbert, "Baunkohlenbergbau und Arbeiterbewegung: Ein Beitrag zur Bergarbeitervolkskunde im nordwestböhmischen Braunkohlenrevier bis zum Ende der österreichisch-ungarischen Monarchie," 1021
- "English America and the Revolution of 1688," by Sosin, 1058
- "The English Churches in a Secular Society," by Cox, 983
- "English Court Culture in the Later Middle Ages," edited by Scattergood and Sherborne (E), 1121
- "The English Labour Movement, 1700-1951," by Brown, 394
- "The English Medieval Landscape," edited by Cantor (E), 787
- "English Radicals and Reformers, 1760-1848," by Royle and Walvin, 979
- "English Zionists and British Jews," by Cohen, 682
- "Ensayos sobre historia de la educación en México," by Vázquez *et al.*, 506
- Enssle, Manfred J. (R), 136
- "Entertaining Satan: Witchcraft and the Culture of Early New England," by Demos, 1316
- "The Entourage of Kaiser Wilhelm II, 1888-1918," by Hull, 700
- "Entscheidung im Westen, 1944," by Ose, 707
- "Die Entstehung des Wohlfahrtsstaates in Grossbritannien und Deutschland, 1850-1950," edited by Mommsen and Mock, 976
- Epp, Frank H., "Mennonites in Canada, 1920-1940: A People's Struggle for Survival," 1110
- Epstein, James (R), 112
- Epstein, James, and Dorothy Thompson, editors, "The Chartist Experience: Studies in Working-Class Radicalism and Culture, 1830-60" (E), 1121
- Epstein, James, "The Lion of Freedom: Feargus O'Connor and the Chartist Movement, 1832-1842," 393
- "Equal Justice under Law: Constitutional Development, 1835-1875," by Hyman and Wiecek, 469
- Erdmann, Karl Dietrich, 231
- "Eric Voegelin's Approach to Thought: A Critical Appraisal," edited by Sandoz (E), 514
- Erich, Haggai, "Ethiopia and Eritrea during the Scramble for Africa: A Political Biography of Räs Alulä, 1875-1897," 1037
- Erich, Haggai, "The Struggle over Eritrea, 1962-1978: War and Revolution in the Horn of Africa," 1305

- "Das Erzbistum Mainz in römischer und fränkischer Zeit," by Heinemeyer, 101
- "Essays in the Political, Economic, and Social History of Colonial Latin America," edited by Spalding (E), 792
- "Essays on American Antebellum Politics, 1840–1860," edited by Maizlish and Kushnia, 1064
- "Essays on Culture and Society in Modern Germany," edited by Stark and Lackner, 1282
- Estes, James Martin, "Christian Magistrate and State Church: The Reforming Career of Johannes Brenz," 1006
- "Ethiopia and Eritrea during the Scramble for Africa," by Erlich, 1037
- "Ethiopia and the Red Sea: The Rise and Decline of the Solomonic Dynasty," by Abir, 727
- ETHNICITY AND ETHNIC HISTORY: Adanir, "Die Makedonische Frage," 1019; Ampalavanar, "The Indian Minority and Political Change in Malaya, 1947–1957," 458; Armstrong, "Nations before Nationalism," 650; Avni, "Spain, the Jews, and Franco," 409; Baily, *The Adjustment of Italian Immigrants in Buenos Aires and New York, 1870–1914*, 281–305; Cohen, "La promotion des Juifs en France à l'époque du Second Empire, 1852–1870," 405; Cohen, "The Politics of Ethnic Survival: Germans in Prague, 1861–1914," 434; De Marco, "Ethnics and Enclaves," 191; Harney and Scarpaci, editors, "Little Italies in North America," 191; Holmquist, editor, "They Chose Minnesota: A Survey of the State's Ethnic Groups," 487; Iancu, "Les juifs en Roumanie, 1866–1919," 142; Kappeler, "Russlands erste Nationalitäten," 718; Klein, "Bolivia: The Evolution of a Multi-Ethnic Society," 510; Klein, *The Integration of Italian Immigrants into the United States and Argentina* (Forum), 306–29; Maas, "The German Community in Renaissance Rome, 1378–1523," 137; Moore, "B'nai B'rith and the Challenge of Ethnic Leadership," 197
- "Ethnics and Enclaves: Boston's Italian North End," by De Marco, 191
- "Études Byzantines et post-Byzantines," edited by Stănescu and Tanaşoca (E), 1357
- "Eugen Richter," by Lorenz, 418
- "Eugene V. Debs," By Salvatore, 1336
- Eulenburg-Hertefeld, Philipp, 130
- "European Direct Investment in the U.S.A. before World War I," by Buckley and Roberts, 490
- "The European Periphery and Industrialization, 1780–1914," by Berend and Ránki, 975
- "European Textile Printers in the Eighteenth Century," by Chapman and Chassagne, 104
- "Eva Perón," by Fraser and Navarro, 513
- Evans, Eric J. (R), 1268
- Evans, R. J. W. (R), 1007
- Evans, Sara M. (R), 1056
- Evergates, Theodore (R), 99
- "The Evolution of American Electoral Systems," by Kleppner *et al.*, 168
- "The Evolution of Irish Nationalist Politics," by Garvin, 121
- "The Evolution of the Modern Commonwealth, 1902–80," by Judd and Slinn, 988
- "The Evolution of Social Insurance, 1881–1981," by Köhler *et al.* (E), 515
- "An Experience of Women," by Robertson, 104
- EXPLORATION AND COLONIZATION: Boardman and Hammond, editors, "The Cambridge Ancient History," vol. 3, part 3: "The Expansion of the Greek World," 966; Boockmann, "Der deutsche Orden," 96; Licate, "Creation of a Mexican Landscape: Territorial Organization and Settlement in the Eastern Puebla Basin," 779; Pethick, "The Nootka Connection: Europe and the Northwest Coast, 1790–1795," 778; Swann, "Tierra Adentro," 1115; Urban, "The Livonian Crusade," 96; Urban, "The Prussian Crusade," 96
- "The Eye That Never Sleeps: A History of the Pinkerton Detective Agency," by Morn, 488
- Eyler, John M. (R), 397
- "Fabianism and Culture," by Britain, 1268
- "Faces of Feminism," by Banks, 83
- Fadiman, Jeffrey A., "An Oral History of Tribal Warfare: The Meru of Mt. Kenya," 1039
- "Fair Sex: Family Size and Structure in Britain, 1900–39," by Gittins, 986
- Faith and Experience in the Thought of John Wesley*, by Dreyer, 12–30
- "Faith and Fraternalism: The History of the Knights of Columbus," by Kauffman, 775
- Falcoff, Mark, and Frederick B. Pike, editors, "The Spanish Civil War, 1936–39: American Hemispheric Perspectives," 1353
- "The Fall of the First British Empire: Origins of the War of American Independence," by Tucker and Hendrickson, 1059
- "The Fall of Natural Man: The American Indian and the Origins of Comparative Ethnology," by Pagden, 1114
- "Family and Divorce in California, 1850–1890," by Griswold, 1080
- "Family and Farm: Agrarian Change and Household Organization in the Loire Valley, 1500–1900," by Shaffer, 686
- FAMILY HISTORY: Behlmer, "Child Abuse and Moral Reform in England, 1870–1908," 1268; Fliegelman, "Prodigals and Pilgrims: The American Revolution against Patriarchal Authority," 465; Gittins, "Fair Sex: Family Size and Structure in Britain, 1900–39," 986; Griswold, "Family and Divorce in California, 1850–1890," 1080; Grubb and Lazerson, "Broken Promises: How Americans Fail Their Children," 762; Lewis, "Royal Succession in Capetian France," 99; Miller, "Fathers and Sons: The Bingham Family and the American Mission," 471; Ryan, "Cradle of the Middle Class: The Family in Oneida County, New York," 752
- "Family Time and Industrial Time: The Relationship between the Family and Work in a New England Industrial Community," by Hareven, 486
- "A Fantasy of Reason: The Life and Thought of William Godwin," by Locke, 392
- Farah, Caesar (R), 724
- "Farm and Factory: The Jesuits and the Development of Agrarian Capitalism in Colonial Quito, 1600–1767," by Cushner, 1350
- Farr, D. M. L. (R), 1112
- "Faschismus Forschung," edited by Eichholtz and Gossweiler, 668
- FASCISM: De Grand, "Italian Fascism," 1019; Eichholtz and Gossweiler, editors, "Faschismus Forschung," 668; Fletcher, "The Search for a New Order: Intellectuals and Fascism in Prewar Japan," 734; Hamerow, *Guilt, Redemption, and Writing German History* (Review Essay), 53–72; Kitchen, "The Coming of Austrian Fascism," 135; Krausnick and Wilhelm, "Die Truppe des Weltanschauungskrieges," 422; Natoli, "La Terza Internazionale e il fascismo, 1919–1923," 384; Saija,

- "Un 'soldino' contro il fascismo: Istituzioni ed élites politiche nella Sicilia del 1923," 428; Smith, "Mussolini," 429; Tallgren, "Hitler und die Helden," 133; Volberg, "Auslandsdeutschum und Drittes Reich," 230; Wagner, "Brothers beyond the Sea: National Socialism in Canada," 220
- "The Fatherhood of God and the Victorian Family," by Fishburn, 192
- "Fathers and Sons: The Bingham Family and the American Mission," by Miller, 471
- Fay, Peter Ward (R), 732
- Feaver, George (R), 984
- Fedorov-Davydov, G. A., "Monety Moskovskoi Rusi: Moskva v bor'be za nezavisimoe i tsentralizovannoe gosudarstvo," 144
- Fedorowicz, J. K., *et al.*, editors, "A Republic of Nobles: Studies in Polish History to 1864" (E), 1123
- Felix, David (R), 963
- "Felix Frankfurter and His Times," by Parrish, 205
- Fell, James E., Jr. (R), 1096
- "Fellah and Townsman in the Middle East," by Baer, 723
- Fellman, Anita Clair, and Michael Fellman, "Making Sense of Self: Medical Advice Literature in Late Nineteenth-Century America," 194
- Felt, Jeremy P. (R), 762
- Femia, Joseph V., "Gramsci's Political Thought: Hegemony, Consciousness, and the Revolutionary Process," 362
- Fender, Stephen, "Plotting the Golden West: American Literature and the Rhetoric of the California Trail," 183
- "Fenland Riots and the English Revolution," by Lindley, 388
- "Ferdinand I of Austria," by Fichtner, 424
- Ferguson, Arthur B. (R), 679
- Ferguson, J. Wilson (R), 989
- Ferling, John E. (R), 177
- Fernandez-Santamaria, J. A. (R), 1000
- Ferrell, Robert H. (R), 1091
- Ferrell, Robert H., "Harry S. Truman and the Modern American Presidency," 1344
- Ferrier, R. W., "The History of the British Petroleum Company," vol. 1: "The Developing Years, 1901–1932," 1270
- Fichtner, Paula Sutter (R), 1289
- Fichtner, Paula Sutter, "Ferdinand I of Austria: The Politics of Dynasticism in the Age of the Reformation," 424
- Field, H. John, "Toward a Programme of Imperial Life: The British Empire at the Turn of the Century," 667
- Field, Phyllis F., "The Politics of Race in New York: The Struggle for Black Suffrage in the Civil War Era," 755
- Fieldhouse, D. K., and Frederick Madden, editors, "Oxford and the Ideal of Commonwealth: Essays Presented to Sir Edgar Williams" (E), 515
- "Fighting Power: German and U.S. Army Performance, 1939–1945," by Van Creveld, 1287
- "Fighting Words: Imperial Censorship and the Russian Press, 1804–1906," by Ruud, 1298
- "The Financial Administration of the Lordship and County of Chester," by Booth, 98
- Fine, John V. A., Jr. (R), 103
- Fingard, Judith, "Jack in Port: Sailortowns of Eastern Canada," 1111
- "Finland and Europe," by Paasivirta, 414
- "Fire in America: A Cultural History of Wildland and Rural Fire," by Pyne, 759
- "The First Urban Christians: The Social World of the Apostle Paul," by Meeks, 1253
- Fischer, Norman, *et al.*, editors, "Continuity and Change in Marxism" (E), 1356
- Fischer-Galati, Stephen, *et al.*, editors, "Romania between East and West: Historical Essays in Memory of Constantin C. Giurescu" (E), 789
- Fishbein, Leslie, "Rebels in Bohemia: The Radicals of *The Masses*, 1911–1917," 767
- Fishburn, Janet Forsythe, "The Fatherhood of God and the Victorian Family: The Social Gospel in America," 192
- Fisher, Alan (R), 718
- Fisher, Chris (R), 1264
- Fisher, Chris, "Custom, Work, and Market Capitalism: The Forest of Dean Colliers, 1788–1888," 394
- Fisher, Michael H. (R), 161
- "Flawed Liberation: Socialism and Feminism," edited by Miller (E), 1356
- Fletcher, R. A., *Cobden as Educator: The Free-Trade Internationalism of Eduard Bernstein, 1899–1914*, 561–78
- Fletcher, William Miles, III, "The Search for a New Order: Intellectuals and Fascism in Prewar Japan," 734
- Fliegelman, Jay, "Prodigals and Pilgrims: The American Revolution against Patriarchal Authority, 1750–1800," 465
- "Florence Nightingale," by Smith, 397
- Flower, Milton E., "John Dickinson: Conservative Revolutionary," 1321
- Flynn, George Q. (R), 1333
- Fogarty, Gerald P., "The Vatican and the American Hierarchy from 1870 to 1965," 1333
- FOLKLORE: Demos, "Entertaining Satan: Witchcraft and the Culture of Early New England," 1316; Du Toit, *No Chosen People: The Myth of the Calvinist Origins of Afrikaner Nationalism and Racial Ideology*, 920–52; Fuller, "Mesmerism and the American Cure of Souls," 1065; Henningsen, "The Witches' Advocate: Basque Witchcraft and the Spanish Inquisition," 692; Herzfeld, "Ours Once More: Folklore, Ideology, and the Making of Modern Greece," 141; Partner, "The Murdered Magicians: The Templars and Their Myth," 970; Rosenberg, "The Code of the West," 477
- "Folkkraft, folkfront, folkdemokrati: De svenska kommunisterna och demokratifrågan, 1943–1977," by Holmberg, 413
- Foner, Philip S. (C), 528
- "For the Cause of Truth: Radicalism in London, 1796–1821," by Hone, 390
- "For Want of Trade: Shipping and the New Jersey Ports, 1680–1783," by Levitt, 463
- Forberger, Rudolf, "Die Industrielle Revolution in Sachsen, 1800–1861," vol. 1, part 1: "Die Revolution der Produktivkräfte in Sachsen, 1800–1830," part 2: "Die Revolution der Produktivkräfte in Sachsen, 1800–1830: Übersichten zur Fabrikentwicklung," 1009
- "Force and Statecraft: Diplomatic Problems of Our Time," by Craig and George, 1246
- "Foreign Policy: USA/USSR," edited by Kegley and McGowan (E), 786
- "Forging the Copper Collar: Arizona's Labor-Management War of 1901–1921," by Byrkit, 1090
- "The Formation of the Early Meiji Legal Order," by Ch'en, 733
- "The Formation of the German Chemical Community, 1720–1795," by Huffbauer, 417
- "Formirovanie sovetskoi nauchnoi intelligentsii, 1917–1927 gg.," by Ivanova, 720
- Formisano, Ronald P. (R), 181

- Forms of Resistance: Songs and Perceptions of Power in Colonial Mozambique*, by Vail and White, 883–919
- Forster, Robert, and Orest Ranum, editors, "Ritual, Religion, and the Sacred" (E), 232
- "Fossi e cavedagne benedicon le campagne," by Poni, 708
- Foster, James C. (C), 528
- Foster, James C., editor, "American Labor in the Southwest: The First One Hundred Years," 200
- Foucault, Michel, 648
- Foucquet, Jean-François, 1042
- "The Foundations of the German Academic Library," by Kunoff, 1283
- Fowler, Loretta, "Arapahoe Politics, 1851–1978: Symbols in Crises of Authority," 480
- Fox, John P., "Germany and the Far Eastern Crisis, 1931–1938: A Study in Diplomacy and Ideology," 421
- Fox, Robert (R), 127
- Fox, Thomas (R), 972
- Frachetta, Girolamo, 709
- Frader, Laura Levine (R), 405
- Fraginals, Manuel Moreno, Herbert S. Klein, and Stanley L. Engerman, *The Level and Structure of Slave Prices on Cuban Plantations in the Mid-Nineteenth Century: Some Comparative Perspectives*, 1201–18
- Fragnoli, Raymond R., "The Transformation of Reform: Progressivism in Detroit—and After, 1912–1933," 1097
- "France et États-Unis, août 1914–avril 1917," by Nouailhat, 766
- "France in Greece during World War I," by Mitrakos, 711
- "Francis I," by Knecht, 122
- Franco, Francisco, 1002
- Frank, R. I. (C), 531
- Frank, R. I. (R), 1253
- Frankenstein, Robert, "Le prix du réarmement français, 1935–1939," 999
- Frankfurter, Felix, 205
- Frantz, John B. (R), 1319
- François, Etienne, "Koblenz im 18. Jahrhundert: Zur Sozial- und Bevölkerungsstruktur einer deutschen Residenzstadt," 700
- Fraser, Derek, editor, "Municipal Reform and the Industrial City," 680
- Fraser, Nicholas, and Marysa Navarro, "Eva Perón," 513
- Fraser, W. Hamish, "The Coming of the Mass Market, 1850–1914," 115
- Fraser, Walter J., Jr., and Winfred B. Moore, Jr., editors, "From the Old South to the New: Essays on the Transitional South," 187
- Frederick, William H. (R), 1312
- Frederick II, 1285
- Free and Coerced Transatlantic Migrations*, by Eltis, 251–80
- Freed, John B. (R), 664, 1258
- Freedeman, Charles E. (R), 690
- "Freedom and Grace: The Life of Asa Mahan," by Madden and Hamilton, 1323
- Freehling, William W. (R), 182
- Frei, Daniel, editor, "Managing International Crises" (E), 1120
- Freidel, Frank (R), 204
- Freidenreich, Harriet Pass (R), 142
- "French Cities in the Nineteenth Century," edited by Merriman (E), 788
- French, David, "British Economic and Strategic Planning, 1905–1915," 118
- Freudenberger, H. (R), 1009
- Freyer, Tony (R), 493, 1335
- "The Friars and the Jews," by Cohen, 93
- Fried, Richard M. (R), 1104
- Friedman, Lawrence J., "Gregarious Saints: Self and Community in American Abolitionism, 1830–1870," 472
- Friend, Theodore (R), 1310
- "Friends and Neighbors: Group Life in America's First Plural Society," edited by Zuckerman, 1319
- "Friends in Conflict: The Anglo-Icelandic Cod Wars," by Jónsson, 669
- "The Friends of Peace: Anti-War Liberalism in England," by Cookson, 111
- Frier, Bruce W. (R), 658
- Frisch, Michael H., and Daniel J. Walkowitz, editors, "Working-Class America: Essays on Labor, Community, and American Society" (E), 1124
- "From Agadir to Armageddon: Anatomy of a Crisis," by Barraclough, 1247
- "From Conquest to Collapse," by Kiernan, 651
- "From Crisis to Crisis: American College Government, 1639–1819," by Herbst, 176
- "From Dictatorship to Democracy," edited by Herz (E), 1120
- "From Hunyadi to Rákóczi: War and Society in Late Medieval and Early Modern Hungary," edited by Bak and Király (E), 1123
- "From Italy to San Francisco," by Cinel, 1088
- "From Muskets to Missiles: Politics and Professionalism in the Chinese Army, 1945–1981," by Jencks, 156
- "From New Deal to New Economics: The American Liberal Response to the Recession of 1937," by May, 495
- "From the Old South to the New," edited by Fraser and Moore, 187
- "From Token to Triumph: The Texas Republicans since 1920," by Olien, 199
- "The Frontier in History," edited by Lamar and Thompson, 84
- Frucht, Richard C., "Dunarea Noastra: Romania, the Great Powers, and the Danube Question, 1914–1921," 712
- Fry, Joseph A., "Henry S. Sanford: Diplomacy and Business in Nineteenth-Century America," 1075
- Fuchs, Rachel G. (R), 1273
- Fulk Nerra, 533–60
- Fuller, Robert C., "Mesmerism and the American Cure of Souls," 1065
- "A Functional Past: The Uses of History in Nineteenth-Century Chile," by Woll, 512
- Fussell, Edwin Sill (R), 183
- Gailey, Harry A. (R), 445
- Gailey, Harry A., "Lugard and the Abeokuta Uprising: The Demise of Egba Independence," 1036
- Gajda, Patricia A., "Postscript to Victory: British Policy and the German-Polish Borderlands, 1919–1925," 120
- Galenson, David W., "White Servitude in Colonial America: An Economic Analysis," 171
- Gallaher, John G. (R), 127
- Ganz, A. Harding (R), 420
- "Garden Cities for America: The Radburn Experience," by Schaffer, 770
- "Garibaldi," by Ugolini, 710
- Garr, Daniel (R), 1080
- Garrett, Clarke (R), 111
- Garside, Patricia L., and Ken Young, "Metropolitan London: Politics and Urban Change, 1837–1981,"

- 1267
 Garthwaite, Gene R. (R), 1303
 Gartner, Lloyd P. (R), 769
 Garver, Bruce M. (R), 713
 Garvin, Tom, "The Evolution of Irish Nationalist Politics," 121
 Gash, Norman (R), 1266
 Gati, Charles, and Jan F. Triska, editors, "Blue-Collar Workers in Eastern Europe," 140
 Gattiker, Annetta (C), 805
 Geggus, David Patrick, "Slavery, War, and Revolution: The British Occupation of Saint Domingue, 1793–1798," 503
 "Geheimdienste und Widerstandsbewegungen im Zweiten Weltkrieg," edited by Schulz (E), 786
 "Die geheime Dynamik autoritärer Diktaturen: Vier Studien über sozialen Wandel in der Franco-Ära," by Waldmann *et al.*, 1002
 Geisert, Brad (R), 1045
 GENEALOGY AND HERALDRY: Ianin, "Novgorodskaja feodal'naia votchina: Istoriko-genealogicheskoe issledovanie," 717; Pinoteau, "Vingt-cinq ans d'études dynastiques," 993
 "The Genesis of Noto," by Tobriner, 1018
 GEOGRAPHY: Green, "The Boundary Hunters: Surveying the 141st Meridian and the Alaska Panhandle," 1333; Orr, "Deer Forests, Landlords, and Crofters," 990
 "The Geopolitics of Leninism," by Page, 76
 "George Washington Carver," by McMurray, 197
 George, Alexander L., and Gordon A. Craig, "Force and Statecraft: Diplomatic Problems of Our Time," 1246
 George, C. H. (R), 670
 George, Henry, 1330
 George, Timothy, "John Robinson and the English Separatist Tradition," 1263
 "Gerald of Wales, 1146–1223," by Bartlett, 968
 "German Air Power in World War I," by Morrow, 1014
 "The German Community in Renaissance Rome, 1378–1523," by Maas, 137
 "German Renaissance Architecture," by Hitchcock, 416
 "The German Social Democrats in Opposition, 1949–1960," by Drummond, 1016
 "German Writers in Soviet Exile, 1933–1945," by Pike, 133
 "The Germans," by Craig, 1281
 "Germany and the Far Eastern Crisis, 1931–1938," by Fox, 421
 Gerson, Jack J. (R), 1043
 Gerteis, Louis S. (R), 755
 "Geschichte und Gegenwart," edited by Boockmann *et al.* (E), 231
 "Das gesellige Jahrhundert: Gesellschaft und Gesellschaften im Zeitalter der Aufklärung," by Im Hof, 972
 Geyer, Dietrich, "Kautskys Russisches Dossier: Deutsche Sozialdemokraten als Treuhänder des russischen Parteivermögens, 1910–1915," 1012
 Ghibaudi, Silvia Rota, "Lavoro e socialismo: Abbozzo di una storia della concezione socialista del lavoro," 963
 Gibbon, Edward, 110
 Gifford, Prosser, and Wm. Roger Louis, editors, "The Transfer of Power in Africa: Decolonization, 1940–1960" (E), 790
 Gilbert, Frond (R), 413
 Gilbert, Bentley B. (R), 118, 1271
 Gilbert, Felix (R), 1259
 Gillingham, John, "The Wars of the Roses: Peace and Conflict in Fifteenth-Century England," 661
 Ginter, Donald F. (R), 978
 Giōrgos, Dertiles, "To zētēma ton trapezōn, 1871–1873: Oikonomikē kai politikē diamachē stēn Ellada tou 19ou aïōna," 431
 Gitins, Diana, "Fair Sex: Family Size and Structure in Britain, 1900–39," 986
 Giurescu, Constantin C., 789
 Glaab, Charles N. (R), 1327
 "Gladstone," vol. 1: "1809–1865," by Shannon, 1266
 "Gladstone—Church, State, and Tractarianism," by Butler, 397
 Glasgow, Roy Arthur (R), 1117
 Glatfelter, R. Edward (R), 1029
 Glazier, Ira A. (R), 360
 Gleason, Walter (R), 146, 437
 Gleason, Walter J., "Moral Idealists, Bureaucracy, and Catherine the Great," 1025
 "Global Deforestation and the Nineteenth-Century World Economy," edited by Tucker and Richards (E), 1120
 "The Glorious Cause: The American Revolution," by Middlekauff, 1060
 "The Glory and Fall of the Ming Dynasty," by Chan, 448
 Glüer, Winfried, "Christliche Theologie in China: T. C. Chao, 1918–1956," 154
 Goble, Danney, and James R. Scales, "Oklahoma Politics: A History," 1094
 Godfrey, John, "1204: The Unholy Crusade," 95
 Godley, Michael R., "The Mandarin-Capitalists from Nanyang: Overseas Chinese Enterprise in the Modernization of China, 1893–1911," 1043
 "The Godly Rebellion: Parisian Cures and the Religious Fronde, 1652–62," by Golden, 402
God's Controversy with Jacobean England, by McGiffert, 1151–74
 "God's Playground: A History of Poland," by Davies, 436
 Godwin, William, 392
 Goheen, Peter G. (R), 1109
 Goitein, S. D. (R), 442
 Golb, Norman, and Omeljan Pritsak, "Khazarian Hebrew Documents of the Tenth Century," 1260
 Golczewski, Mechthild, "Der Balkan in deutschen und österreichischen Reise- und Erlebnisberichten, 1912–1918," 1294
 Goldberg, Hillel (R), 80
 Golden, Richard M., "The Godly Rebellion: Parisian Cures and the Religious Fronde, 1652–62," 402
 Goldfield, David R., "Cotton Fields and Skyscrapers: Southern City and Region, 1607–1980," 1314
 Gollin, Alfred (R), 107
 Golob, Eugene O. (R), 358
 Gomme, A. W., *et al.*, "A Historical Commentary on Thucydides," vol. 5: "Book VIII," 367
 González, Julio Heise, "El Periodo parlamentario, 1861–1925," vol. 2: "Democracia y gobierno representativo en el periodo parlamentario," 1118
 "Good Wives: Image and Reality in the Lives of Women in Northern New England, 1650–1750," by Ulrich, 173
 Goodman, Paul (R), 461
 Goodway, David, "London Chartism, 1838–1848," 112
 Gordon, Bertram M. (R), 1295
 Gordon, Leonard A. (R), 1051
 Gossman, Lionel (R), 402
 Gossweiler, Kurt, and Dietrich Eichholtz, editors, "Faschismus Forschung: Positionen, Probleme, Polemik," 668
 Gottfried, Robert S., "Bury St. Edmunds and the

- Urban Crisis, 1290–1539,” 375
- Gould, J. D., *Comment on Klein, The Integration of Italian Immigrants into the United States and Argentina* (Forum), 334–38
- “Government and Society in Central America,” by Wortman, 780
- “The Government of Victorian London, 1855–1889,” by Owen, 1267
- “Governmental Secrecy and the Founding Fathers,” by Hoffman, 179
- “Governor Philip F. La Follette, the Wisconsin Progressives, and the New Deal,” by Miller, 1097
- Goy, Joseph, and Celina Bobińska, editors, “Les Pyrénées et les Carpates, XVI^e–XX^e siècles” (E), 233
- Goy, Joseph, and Emmanuel Le Roy Ladurie, “Tithes and Agrarian History from the Fourteenth to the Nineteenth Centuries: An Essay in Comparative History,” 1240
- Gragg, Larry D. (C), 530
- Graham, Leroy, “Baltimore: The Nineteenth-Century Black Capital,” 1324
- Graham, Loren R. (R), 720
- Graham, Loren R., “Between Science and Values,” 359
- Graham, Otis L., Jr. (R), 494
- Graham, W. Fred (R), 380
- “Gramsci’s Political Thought,” by Femia, 362
- Gran, Peter (R), 443
- “The Grand Domestic Revolution: A History of Feminist Designs for American Homes, Neighborhoods, and Cities,” by Hayden, 1080
- Gransden, Antonia, “Historical Writing in England,” vol. 2: “C. 1307 to the Early Sixteenth Century,” 1256
- Grant, H. Roger, “Self-Help in the 1890s Depression,” 1081
- Grantham, George W. (R), 686
- Gras, Solange, and Christian Gras, “La révolte des régions d’Europe occidentale de 1916 à nos jours,” 385
- Grassl, Herbert, “Sozialökonomische Vorstellungen in der kaiserzeitlichen griechischen Literatur (1.–3. Jh. n. Chr.),” 1253
- Gravier, Charles, Comte de Vergennes, 996
- Grayson, Benson Lee, “Saudi-American Relations,” 499
- “The Great Map of Mankind: Perceptions of New Worlds in the Age of Enlightenment,” by Marshall and Williams, 971
- “The Great Television Race: A History of the American Television Industry, 1925–1941,” by Udelson, 492
- Greaves, Richard L. (C), 1141
- Greaves, Richard L. (R), 388
- “The Greek State at War,” vol. 3: “Religion,” by Pritchett, 88
- Green, Arnold H. (R), 723
- Green, Lewis, “The Boundary Hunters: Surveying the 141st Meridian and the Alaska Panhandle,” 1333
- Green, Michael D., “The Politics of Indian Removal: Creek Government and Society in Crisis,” 478
- Green, William A. (R), 1114
- Greenberg, Dolores (C), 801
- Greene, Jack P. (R), 172, 1060
- Greenhalgh, Peter, “Pompey: The Republican Prince,” 656
- Greenhalgh, Peter, “Pompey: The Roman Alexander,” 656
- Greenough, Paul R., “Prosperity and Misery in Modern Bengal: The Famine of 1943–1944,” 1051
- Greenstein, Fred I., “The Hidden-Hand Presidency: Eisenhower as Leader,” 1345
- “Gregarious Saints: Self and Community in American Abolitionism, 1830–1870,” by Friedman, 472
- Gregory (“the Great”), Pope, 92
- Grieder, Jerome B. (R), 155
- Griffith, Robert (R), 1345
- Griffith, Robert, and Carol G. Thomas, editors, “The City-State in Five Cultures,” 81
- Griswold, Robert L., “Family and Divorce in California, 1850–1890: Victorian Illusions and Everyday Realities,” 1080
- Grohmann, Alberto, “Città e territorio tra medioevo ed età moderna: Perugia, secc. XIII–XVI,” vol. 1: “La città,” vol. 2: “Il territorio,” 707
- Gronewold, Sue, “Beautiful Merchandise: Prostitution in China, 1860–1936,” 1044
- Gross, David (R), 133
- Gross, Hanns (R), 1296
- Gross, Robert A. (R), 752
- Grossberg, Kenneth Alan, “Japan’s Renaissance: The Politics of the Muromachi Bakufu,” 454
- Grubb, W. Norton, and Marvin Lazerson, “Broken Promises: How Americans Fail Their Children,” 762
- Gruenwald, Ithamar, “Apocalyptic and Merkavah Mysticism,” 371
- Grünthal, Günther, “Parlamentarismus in Preussen, 1848/49–1857/58: Preussischer Konstitutionalismus—Parlament und Regierung in der Reaktionsära,” 1008
- “Guarding the Forests of Southern California,” by Lockmann, 202
- Gueiros Vieira, David, “O protestantismo, a maçonaria e a questão religiosa no Brasil,” 228
- Guha, Ranajit, editor, “Writings on South Asian History and Society” (E), 1124
- Guilt, Redemption, and Writing German History* (Review Essay), by Hamerow, 53–72
- Gundersen, Joan R. (R), 1057
- Gungwu, Wang (R), 457
- Gurevich, A. Ia., “Problemy srednevekovoi narodnoi kul’tury,” 666
- Gurock, Jeffrey S. (R), 1331
- Gutman, Herbert G., and Ira Berlin, *Natives and Immigrants, Free Men and Slaves: Urban Workingmen in the Antebellum South, 1175–1200*
- Gutman, Yisrael, “The Jews of Warsaw, 1939–1943: Ghetto, Underground, Revolt,” 716
- Gutteridge, Richard (R), 703
- Haas, Francis J., 1099
- Habib, Irfan, and Tapan Raychaudhuri, editors, “The Cambridge Economic History of India,” vol. 1: “C. 1200–c. 1750,” 735
- “La hacienda del Antiguo Régimen,” by Artola, 1000
- Haddad, Robert M. (R), 431
- Haffenden, Philip S. (R), 462
- Hager, Hellmut (R), 416
- Haider-Pregler, Hilde, “Des sittlichen Bürgers Abendschule: Bildungsanspruch und Bildungsauftrag des Berufstheaters im 18. Jahrhundert,” 1284
- Haim, Sylvia G., and Elie Kedourie, editors, “Palestine and Israel in the Nineteenth and Twentieth Centuries” (E), 516
- Hale, Charles A. (R), 505
- Hall, John W. (R), 454
- Hall, Michael G. (R), 1058
- Hall, Peter Dobkin, “The Organization of American Culture, 1700–1900: Private Institutions, Elites, and

- the Origins of American Nationality," 165
- Hallenbeck, Jan T., "Pavia and Rome: The Lombard Monarchy and the Papacy in the Eighth Century," 665
- Halperin, David J., "The Merkabah in Rabbinic Literature," 371
- Halperin-Donghi, Tulio, *Comment on Klein, The Integration of Italian Immigrants into the United States and Argentina* (Forum), 338-42
- Hamalainen, Pekka Kalevi (R), 414
- Hambrick-Stowe, Charles E., "The Practice of Piety: Puritan Devotional Disciplines in Seventeenth-Century New England," 1317
- Hamby, Alonzo L. (R), 1106
- Hamerow, Theodore S., *Guilt, Redemption, and Writing German History* (Review Essay), 53-72
- Hamilton, Bernard (R), 95
- Hamilton, Bernard, "The Latin Church in the Crusader States: The Secular Church," 96
- Hamilton, Charles D. (R), 88
- Hamilton, James E., and Edward H. Madden, "Freedom and Grace: The Life of Asa Mahan," 1323
- Hamilton, Nora, "The Limits of State Autonomy: Post-Revolutionary Mexico," 780
- Hamilton, Richard F., "Who Voted for Hitler?" 705
- Hammer, C. Pohl (C), 249
- Hammond, N. G. L., "Alexander the Great: King, Commander, and Statesman," 1252
- Hammond, N.G. L., and John Boardman, editors, "The Cambridge Ancient History," vol. 3, part 3: "The Expansion of the Greek World, Eighth to Sixth Centuries B.C.," 966
- Hampel, Robert L., "Temperance and Prohibition in Massachusetts, 1813-1852," 1321
- Hampson, Norman (R), 125
- "Han Civilization," by Zhongshu, 152
- "Die Handelskammern der Rheinprovinz in der zweiten Hälfte des 19. Jahrhunderts," by Dorsch, 1009
- Handlin, Oscar, and Lilian Handlin, "A Restless People: Americans in Rebellion, 1770-1787," 178
- Handy, Robert T. (R), 1084
- Hane, Mikiso, "Peasants, Rebels, and Outcasts: The Underside of Modern Japan," 157
- Hanley, Susan B. (R), 1307
- "The Hanlin Academy," by Lui, 1041
- "Hannah Arendt," by Young-Bruehl, 75
- Hanning, Robert W., and David Rosand, editors, "Castiglione: The Ideal and the Real in Renaissance Culture" (E), 1122
- Hanson, Carl A. (R), 410
- Hanson, Joanna K. M., "The Civilian Population and the Warsaw Uprising of 1944," 1022
- "The Harder We Run: Black Workers since the Civil War," by Harris, 768
- Hardiman, David, "Peasant Nationalists of Gujarat: Kheda District, 1917-1934," 456
- Harding, Harry, "Organizing China: The Problem of Bureaucracy, 1949-1976," 453
- Hareven, Tamara K., "Family Time and Industrial Time: The Relationship between the Family and Work in a New England Industrial Community," 486
- Hargreaves, John D. (R), 727
- Harlow, Neal, "California Conquered: War and Peace on the Pacific, 1846-1850," 1068
- Harms, Robert W., *The Wars of August: Diagonal Narratives in African History*, 809-34
- Harna, Josef, et al., "Materiály k politickým, hospodářským a sociálním dějinám Československa v letech 1918-1929," 712
- Harnetty, Peter (R), 1051
- Harney, Robert F., and J. Vincenza Scarpaci, editors, "Little Italies in North America," 191
- "Harold L. Ickes," by Lear, 206
- Harris, Barbara J. (R), 1339
- Harris, Howell John, "The Right to Manage: Industrial Relations Policies of American Business in the 1940s," 1102
- Harris, Robert D. (R), 996
- Harris, William A., "The Harder We Run: Black Workers since the Civil War," 768
- Harris, William V. (R), 655
- Harrison, Brian (R), 458, 986
- Harrison, James P. (R), 459
- Harrison, James Pinckney, "The Endless War: Fifty Years of Struggle in Vietnam," 740
- Harrod, Frederick S. (R), 210
- "Harry S. Truman and the Modern American Presidency," by Ferrell, 1344
- Hartgrove, J. Dane (R), 1300
- Harvey, David, "The Limits to Capital," 1243
- Hassenpflug-Elzholz, Eila, "Böhmen und die böhmischen Stände in der Zeit des beginnenden Zentralismus: Eine Strukturanalyse der böhmischen Adelsnation um die Mitte des 18. Jahrhunderts," 1296
- Hassler, Warren W., Jr., "With Shield and Sword: American Military Affairs, Colonial Times to the Present," 1315
- Hast, Adele, "Loyalism in Revolutionary Virginia: The Norfolk Area and the Eastern Shore," 177
- Hatch, Nathan O., and Mark A. Noll, editors, "The Bible in America: Essays in Cultural History," 744
- Haug, C. James, "Leisure and Urbanism in Nineteenth-Century Nice," 997
- Hauner, Milan (R), 1287
- Hauner, Milan, "India in Axis Strategy: Germany, Japan, and Indian Nationalists in the Second World War," 739
- Havard, William C., and Walter Sullivan, editors, "A Band of Prophets: The Vanderbilt Agrarians after Fifty Years" (E), 235
- Havelock, Eric A., "The Literate Revolution in Greece and Its Cultural Consequences" (E), 516
- Havens, Thomas R. H. (R), 157
- Havens, Thomas R. H., "Artist and Patron in Postwar Japan: Dance, Music, Theater, and the Visual Arts, 1955-1980," 735
- Haws, Charles H. (R), 683
- Hay, Margaret Jean, and Marcia Wright, editors, "African Women and the Law: Historical Perspectives" (E), 1123
- Hayden, Dolores, "The Grand Domestic Revolution: A History of Feminist Designs for American Homes, Neighborhoods, and Cities," 1080
- Heale, Michael J., "The Presidential Quest: Candidates and Images in American Political Culture, 1787-1852," 468
- HEALTH, DISEASE, AND NUTRITION: Coleman, "Death Is a Social Disease," 996; Cornebise, "Typhus and Doughboys," 1335; Dowling, "City Hospitals," 764; Greenough, "Prosperity and Misery in Modern Bengal," 1051; Leavitt, "The Healthiest City: Milwaukee and the Politics of Health Reform," 488; Rosner, "A Once Charitable Enterprise: Hospitals and Health Care in Brooklyn and New York, 1885-1915," 763; Sussman, "Selling Mothers' Milk: The Wet-Nursing Business in France, 1715-1814," 688; Whorton, "Crusaders for Fitness," 1066
- "The Healthiest City: Milwaukee and the Politics of Health Reform," by Leavitt, 488
- Heckart, Beverly (R), 131

- Heinemeyer, Karl, "Das Erzbistum Mainz in römischer und fränkischer Zeit," vol. 1: "Die Anfänge der Diözese Mainz," 101
- Heller, Agnes, "A Theory of History," 358
- Heller, Henry (R), 699
- Heller, Joseph (R), 682
- Hellie, Richard, "Slavery in Russia, 1450–1725," 1023
- Hendricks, Charles (C), 531
- Hendrickson, David C., and Robert W. Tucker, "The Fall of the First British Empire: Origins of the War of American Independence," 1059
- Hendrickson, Kenneth E., Jr. (R), 776
- Henningsen, Gustav, "The Witches' Advocate: Basque Witchcraft and the Spanish Inquisition (1609–1614)," 692
- "Henry S. Sanford," by Fry, 1075
- Henry, Patrick (R), 369
- "Herbert Marcuse and the Art of Liberation," by Katz, 963
- Herbst, Jürgen, "From Crisis to Crisis: American College Government, 1639–1819," 176
- Herring, George C. (R), 740
- Herz, John H., editor, "From Dictatorship to Democracy: Coping with the Legacies of Authoritarianism and Totalitarianism" (E), 1120
- Herzfeld, Michael, "Ours Once More: Folklore, Ideology, and the Making of Modern Greece," 141
- Hewitt, George R., "Scotland under Morton, 1572–80," 683
- Hewlett, Sylvia Ann, and Richard S. Weinert, editors, "Brazil and Mexico: Patterns in Late Development" (E), 1125
- Heyck, T. W., "The Transformation of Intellectual Life in Victorian England," 981
- Heydenreuter, Reinhard, "Der landesherrliche Hofrat unter Herzog und Kurfürst Maximilian I. von Bayern, 1598–1651," 1007
- "The Hidden-Hand Presidency: Eisenhower as Leader," by Greenstein, 1345
- "Hideyoshi," by Berry, 1046
- Higham, John (C), 804
- "The Highest Stage of White Supremacy," by Cell, 965
- Hildebrand, Klaus, and Karl Ferdinand Werner, editors, "Deutschland und Frankreich, 1936–1939" (E), 788
- "Hilfe + Handel = Frieden? Die Bundesrepublik in der Dritten Welt," edited by Steinweg (E), 789
- Hilpert, Hans-Eberhard, "Kaiser- und Papstbriefe in den *Chronica majora* des Matthaeus Paris," 102
- Himmelberg, Robert F. (R), 202
- Hinckley, Ted C. (R), 1333
- Hinckley, Ted C., "Alaskan John G. Brady: Missionary, Businessman, Judge, and Governor, 1878–1918," 1328
- Hipper, Franz von, Admiral, 420
- Hirschfeld, Gerhard, and Wolfgang J. Mommsen, editors, "Social Protest, Violence, and Terror in Nineteenth- and Twentieth-Century Europe" (E), 1356
- Hiss, Alger, 1105
- The Historian and the Study of International Relations*, by Craig, 1–11
- "A Historical Commentary on Thucydides," vol. 5: "Book VIII," by Gomme *et al.*, 367
- "Historical Writing in England," vol. 2: "C. 1307 to the Early Sixteenth Century," by Gransden, 1256
- HISTORIOGRAPHY AND HISTORICAL METHODS: Barber, "Soviet Historians in Crisis, 1928–1932," 148; Brucker, *Tales of Two Cities: Florence and Venice in the Renaissance* (Review Essay), 599–616; Cohen, "Josephus in Galilee and Rome," 89; Craig, *The Historian and the Study of International Relations*, 1–11; Du Toit, *No Chosen People: The Myth of the Calvinist Origins of Afrikaner Nationalism and Racial Ideology*, 920–52; Gomme *et al.*, "A Historical Commentary on Thucydides," vol. 5: "Book VIII," 367; Gransden, "Historical Writing in England," vol. 2: "C. 1307 to the Early Sixteenth Century," 1256; Hamerow, *Guilt, Redemption, and Writing German History* (Review Essay), 53–72; Harms, *The Wars of August: Diagonal Narratives in African History*, 809–34; Heller, "A Theory of History," 358; Hunter, "Past and Process in Herodotus and Thucydides," 87; Kessler, "Politik, Kultur und Gesellschaft in Kroatien und Slawonien in der ersten Hälfte des 19. Jahrhunderts," 1020; Kraditor, "The Radical Persuasion, 1890–1917," 491; Levandovskii, "Iz istorii krizisa russkoi burzhuažno-liberal'noi istoriografii," 1299; Van der Dussen, "History as a Science: The Philosophy of R. G. Collingwood," 73; Woll, "A Functional Past: The Uses of History in Nineteenth-Century Chile," 512; Yeager, "Barros Arana's *Historia jeneral de Chile*," 229
- "History and Imagination," edited by Lloyd-Jones *et al.* (E), 231
- History as Current Events: Recent Works on the German Revolution of 1848* (Review Essay), by Mattheisen, 1219–37
- "History as a Science: The Philosophy of R. G. Collingwood," by Van der Dussen, 73
- "A History of British Industrial Relations, 1875–1914," edited by Wrigley (E), 515
- "The History of the British Petroleum Company," vol. 1: "The Developing Years, 1901–1932," by Ferrier, 1270
- "A History of the Church in Latin America," by Dussel, 778
- "History of Education and Culture in America," by Button and Provenzo, 1055
- "A History of English Forestry," by James, 108
- "A History of European Integration," vol. 1: "1945–1947," by Lippens, 365
- "A History of the Highland Clearances," by Richards, 400
- "The History of Indiana," vol. 5: "Indiana through Tradition and Change," by Madison, 773
- "A History of Metals in Colonial America," by Mulholland, 1318
- "A History of Sri Lanka," by De Silva, 163
- "A History of the United Nations," vol. 1: "The Years of Western Domination, 1945–1955," by Luard, 366
- Hitchcock, Henry-Russell, "German Renaissance Architecture," 416
- "Hitler und die Helden," by Tallgren, 133
- Hitler, Adolf, 705
- "Hitlers Politische Soldaten," by Wegner, 1287
- Hoak, Dale (R), 670
- Hobart, Michael E., "Science and Religion in the Thought of Nicholas Malebranche," 75
- Hoffman, Abraham (R), 201
- Hoffman, Daniel N., "Governmental Secrecy and the Founding Fathers: A Study in Constitutional Controls," 179
- Hoffman, Ronald, and Peter J. Albert, editors, "Sovereign States in an Age of Uncertainty," 1061
- "De höga officerarna," by Cavallie, 695
- "Die Hohe Schule Herborn in ihrer Frühzeit, 1584–1660," by Menk, 1007
- Holl, Karl, and Wolfram Wette, editors, "Pazifismus in der Weimarer Republik: Beiträge zur historischen Friedensforschung" (E), 234
- Hollander, Paul (R), 1337

- Holley, Donald (R), 1341
 Hollinger, David A. (R), 359
 "A Hollow Threat: Strategic Air Power," by Borowski, 216
 Holloway, Joseph E., "Liberian Diplomacy in Africa: A Study of Inter-African Relations," 1036
 Holly, James Theodore, 1073
 "Hollywood as Historian," edited by Rollins (E), 1358
 Holmberg, Håakan, "Folkmakt, folkfront, folkdemokrati: De svenska kommunisterna och demokratifrågan, 1943–1977," 413
 Holmes, Clive (R), 388
 Holmes, John W., "The Shaping of Peace: Canada and the Search for World Order, 1943–1957," 221
 Holmes, Peter, "Resistance and Compromise: The Political Thought of the Elizabethan Catholics," 671
 Holmquist, June Drenning, editor, "They Chose Minnesota: A Survey of the State's Ethnic Groups," 487
 Holt, Michael F. (R), 473
 Holt, Stephen, "Manning Clark and Australian History, 1915–1963," 742
 Holton, Gerald, and Yehuda Elkana, editors, "Albert Einstein: Historical and Cultural Perspectives" (E), 514
 "The Holy War in Los Altos," by Tuck, 1116
 Hone, J. Ann, "For the Cause of Truth: Radicalism in London, 1796–1821," 390
 Honoré, Tony, "Emperors and Lawyers," 658
 Hooglund, Eric J., "Land and Revolution in Iran, 1960–1980," 1033
 Hook, Judith (R), 709
 "Hooligans or Rebels?" by Humphries, 116
 Hoover, J. Edgar, 953–60
 Höpfl, Harro, "The Christian Polity of John Calvin," 698
 Hopkins, James K., "A Woman to Deliver Her People: Joanna Southcott and English Millenarianism in an Era of Revolution," 392
 Horak, Stephan M. (R), 143, 718
 Hornung, Erik, "Conceptions of God in Ancient Egypt: The One and the Many," 1250
 Horwitz, Henry (R), 390
 Hosmer, Charles B., Jr., "Preservation Comes of Age: From Williamsburg to the National Trust, 1926–1949," 1100
 Hou, Chi-Ming (R), 1043
 Housley, Norman, "The Italian Crusades: The Papal Angevin Alliance and the Crusades against Christian Lay Powers, 1254–1343," 378
 Hovannisian, Richard G., "The Republic of Armenia," vol. 2: "From Versailles to London, 1919–1920," 1032
 Howard, Henry, Earl of Northampton, 1262
 Hoyt, William B. (C), 249
 "Hrabanus Maurus," edited by Kottje and Zimmermann, 969
 Huard, Raymond, "La préhistoire des partis: Le mouvement républicain en Bas-Languedoc, 1848–1881," 687
 Huber, Thomas M., "The Revolutionary Origins of Modern Japan," 157
 Hucker, Charles O. (R), 448
 Hudson, Michael C. (R), 444
 Hufbauer, Karl, "The Formation of the German Chemical Community, 1720–1795," 417
 Hughes, Kathleen, 787
 Hull, Isabel V. (R), 130
 Hull, Isabel V., "The Entourage of Kaiser Wilhelm II, 1888–1918," 700
 Hull, Richard W. (R), 447
 Hume, Ivor Noël, "Martin's Hundred," 170
 Hume, Leslie Parker, "The National Union of Women's Suffrage Societies, 1897–1914," 986
 Humphries, Stephen, "Hooligans or Rebels? An Oral History of Working-Class Childhood and Youth, 1889–1939," 116
 "The Hundred Years War," by Seward, 968
 Hunt, Barry D., "Sailor-Scholar: Admiral Sir Herbert Richmond, 1871–1946," 987
 Hunt, John Dixon (R), 980
 Hunt, Richard Morris, 195
 Hunter, Virginia, "Past and Process in Herodotus and Thucydides," 87
 Hurst, G. Cameron, III (R), 455
 Hutton, Patrick H., "The Cult of the Revolutionary Tradition: The Blanquists in French Politics, 1864–1893," 126
 Huxley, Aldous, 1245
 Huzar, Eleanor, G. (R), 656
 Hyman, Harold D., and William M. Wiecek, "Equal Justice under Law: Constitutional Development, 1835–1875," 469
 Iancu, Carol, "Les juifs en Roumanie, 1866–1919: De l'exclusion à l'émancipation," 142
 Ianin, V. L., "Novgorodskaja feodal'naia votchina: Istoriko-genealogicheskoe issledovanie," 717
 Iatrides, John O. (R), 213
 Ickes, Harold L., 206
 "Ideas, Faiths, and Feelings: Essays on American Intellectual and Religious History," edited by May (E), 1358
 "L'idée républicaine en France, 1789–1924," by Nicolet, 998
 "Ideology and Conspiracy: Aspects of Jacobitism, 1689–1759," edited by Cruickshanks (E), 788
 "Ideology and Development in Africa," by Young, 445
 "Ideology and Experience: Antisemitism in France at the Time of the Dreyfus Affair," by Wilson, 691
 Ikle, Frank W. (R), 1047, 1306
 Im Hof, Ulrich, "Das gesellige Jahrhundert: Gesellschaft und Gesellschaften im Zeitalter der Aufklärung," 972
 Immerman, Richard H., "The CIA in Guatemala: The Foreign Policy of Intervention," 781
 IMMIGRATION AND EMIGRATION: Baily, *The Adjustment of Italian Immigrants in Buenos Aires and New York, 1870–1914*, 281–305; Bodnar *et al.*, "Lives of Their Own: Blacks, Italians, and Poles in Pittsburgh, 1900–1960," 1089; Cinel, "From Italy to San Francisco," 1088; Cross and Sandos, "Across the Border: Rural Development in Mexico and Recent Migration to the United States," 226; Dinnerstein, "America and the Survivors of the Holocaust," 215; Eltis, *Free and Coerced Transatlantic Migrations*, 251–80; Irick, "Ch'ing Policy toward the Coolie Trade, 1847–1878," 732; Klein, *The Integration of Italian Immigrants into the United States and Argentina* (Forum), 306–29; Shankman, "Ambivalent Friends: Afro-Americans View the Immigrant," 768; Vernassa, "Emigrazione, diplomazia et cannoniere: L'intervento italiano in Venezuela," 227; Zunz, "The Changing Face of Inequality," 1088
 "The Impact of English Towns, 1700–1800," by Corfield, 676
 "An Impartial Umpire: Industrial Relations and the Canadian State," by Craven, 219
 "The Impasse of European Communism," by Boggs,

- 362
 "Imperial Trusteeship and Political Evolution in Kenya, 1923–1963," by Kamoche, 151
 "Imperiale Herrschaft und nationales Interesse: 'Constructive Imperialism' oder Freihandel in Grossbritannien vor dem Ersten Weltkrieg," by Mock, 1269
 IMPERIALISM: Baumgart, "Imperialism: The Idea and Reality of British and French Colonial Expansion," 667; Field, "Toward a Programme of Imperial Life: The British Empire," 667; Geggus, "Slavery, War, and Revolution: The British Occupation of Saint Domingue, 1793–1798," 503; Miller, "Benevolent Assimilation: The American Conquest of the Philippines, 1899–1903," 1334; Mock, "Imperiale Herrschaft und nationales Interesse: 'Constructive Imperialism' oder Freihandel in Grossbritannien vor dem Ersten Weltkrieg," 1269; Schneider, "An Empire for the Masses: The French Popular Image of Africa, 1870–1900," 1274
 "Imperialism: The Idea and Reality of British and French Colonial Expansion," by Baumgart, 667
 "Imperialismus und Kolonialmission: Kaiserliches Deutschland und koloniales Imperium," edited by Bade, 1011
 "In Defense of La Raza: The Los Angeles Mexican Consulate and the Mexican Community, 1929 to 1936," by Balderrama, 774
 "In Defiance of the Law: The Standing-Army Controversy, the Two Constitutions, and the Coming of the American Revolution," by Reid, 177
 "In the Eyes of the Law: Women, Marriage, and Property in Nineteenth-Century America," by Basch, 485
 "In Defiance of Oligarchy: The Tory Party, 1714–60," by Colley, 390
 "In the Shadow of Florence: Provincial Society in Renaissance Pescia," by Brown, 1292
 "In the Vanguard of Reform: Russia's Enlightened Bureaucrats, 1825–1861," by Lincoln, 1298
 "In Whose Best Interest? Child Welfare Reform in the Progressive Era," by Tiffin, 762
 "The Incorporation of America: Culture and Society in the Gilded Age," by Trachtenberg, 193
 "La independencia en el Perú," by Bonilla *et al.* (E), 235
 "India in Axis Strategy," by Hauner, 739
 "The Indian Minority and Political Change in Malaya, 1945–1957," by Ampalavanar, 458
 "Indian Summer," by Irving, 738
 "Indians of the Pacific Northwest," by Ruby and Brown, 479
 "En industri kommer till stan: Hudiksvall och trävaruindustrin, 1855–1876," by Lundbäck, 695
 "Industrial Archaeology in the British Isles," by Butt and Donnachie, 114
 INDUSTRIALIZATION: Berend and Ránki, "The European Periphery and Industrialization, 1780–1914," 975; Chapman and Chassagne, "European Textile Printers in the Eighteenth Century," 104; Cobb, "The Selling of the South: The Southern Crusade for Industrial Development," 1101; Eller, "Miners, Millhands, and Mountaineers: Industrialization of the Appalachian South," 771; Englisch, "Baunkohlenbergbau und Arbeiterbewegung," 1021; Forberger, "Die Industrielle Revolution in Sachsen, 1800–1861," 1009; Hareven, "Family Time and Industrial Time," 486; Johnson, "MITI and the Japanese Miracle," 158; Lundbäck, "En industri kommer till stan: Hudiksvall och trävaruindustrin, 1855–1876," 695; Mayr and Post, editors, "Yankee Enterprise," 468; Trebilcock, "The Industrialization of the Continental Powers, 1780–1914," 974; Trinder, "The Making of the Industrial Landscape," 1264; Triska and Gati, editors, "Blue-Collar Workers in Eastern Europe," 140; Zunz, "The Changing Face of Inequality," 1088
 "The Industrialization of the Continental Powers, 1780–1914," by Trebilcock, 974
 "Die Industrielle Revolution in Sachsen, 1800–1861," by Forberger, 1009
 "Infamy: Pearl Harbor and Its Aftermath," by Toland, 496
 "L'Inghilterra e il pensiero politico di Montesquieu," by Landi, 381
 INSTITUTIONAL HISTORY: Armstrong, "The Rise of the International Organisation," 1250; Blessing, "Staat und Kirche in der Gesellschaft," 1010; Booth, "The Financial Administration of the Lordship and County of Chester, 98; Bremner and Reichard, editors, "Reshaping America: Society and Institutions, 1945–1960," 1106; Gleason, "Moral Idealists, Bureaucracy, and Catherine the Great," 1025; James, "A History of English Forestry," 108; Lincoln, "In the Vanguard of Reform: Russia's Enlightened Bureaucrats, 1825–1861," 1298; Luard, "A History of the United Nations," vol. 1: "The Years of Western Domination, 1945–1955," 366; Lintinen, "Kuninkaan 'perintöä ja omaa' (arv och eget)," 696; Morn, "The Eye That Never Sleeps: A History of the Pinkerton Detective Agency," 488; Nelson, "The Roots of American Bureaucracy, 1830–1900," 1063; Perlmutter, "Modern Authoritarianism," 652; Ultee, "The Abbey of St. Germain des Prés in the Seventeenth Century," 123
 "Instrumentalism and American Legal Theory," by Summers, 1335
 "The Insular Cases: The Role of the Judiciary in American Expansionism," by Kerr, 187
The Integration of Italian Immigrants into the United States and Argentina (Forum), by Klein, 306–29
 INTELLECTUAL HISTORY: Ausmus, "The Polite Escape: On the Myth of Secularization," 358; Berman, "The Reenchantment of the World," 359; Blessing, "Staat und Kirche in der Gesellschaft," 1010; Christoff, "An Introduction to Nineteenth-Century Russian Slavophilism," vol. 3: "K. S. Aksakov," 1026; Dahlstrand, "Amos Bronson Alcott," 761; Friedman, "Gregarious Saints: Self and Community in American Abolitionism, 1830–1870," 472; Graham, "Between Science and Values," 359; Heyck, "The Transformation of Intellectual Life in Victorian England," 981; Jackson, "The Oriental Religions and American Thought," 193; Katz, "Herbert Marcuse and the Art of Liberation," 963; Kelly, "Mikhail Bakunin: A Study in the Psychology and Politics of Utopianism," 1243; Landi, "L'Inghilterra il pensiero politico di Montesquieu," 381; Leroy, "Péguy entre l'ordre et la révolution," 407; Lippy, "Seasonable Revolutionary: The Mind of Charles Chauncy," 749; Lough, "The *Philosophes* and Post-Revolutionary France," 402; Lunn, "Marxism and Modernism," 1245; Marshall and Williams, "The Great Map of Mankind: Perceptions of New Worlds in the Age of Enlightenment," 971; Morrison, "The Mimetic Tradition of Reform in the West," 646; O'Neill, "A Better World: The Great Schism, Stalinism, and the American Intellectuals," 1337; Schleich, "Aufklärung und Revolution," 402; Shils, "Tradition," 961; Singal, "The War Within: From Victorian to Modernist Thought in the South," 772; Sironneau,

- "Sécularisation et religions politiques," 358;
 Stranges, "Electrons and Valence," 964; Stuart,
 "War and American Thought from the Revolution
 to the Monroe Doctrine," 752; Thomas,
 "Alternative America," 1330; Wood, "Men against
 Time," 1245; Wylie, "The Emergence of Maoism,"
 451
- INTERNATIONAL RELATIONS: Brune, "The Origins of
 American National Security Policy," 1093; Bryson,
 "Seeds of Mideast Crisis: The United States
 Diplomatic Role in the Middle East during World
 War II," 213; Craig, *The Historian and the Study of
 International Relations*, 1-11; Douglas, "New
 Alliances, 1940-41," 364; Falcoff and Pike, editors,
 "The Spanish Civil War, 1936-39: American
 Hemispheric Perspectives," 1353; Fletcher, *Cobden
 as Educator: The Free-Trade Internationalism of Edward
 Bernstein, 1899-1914*, 561-78; Holmes, "The
 Shaping of Peace: Canada and the Search for
 World Order, 1943-1957," 220; Immerman, "The
 CIA in Guatemala," 781; Jacobson, *Is There a New
 International History of the 1920s?* (Review Essay),
 617-45; Joseph, "Revolution from Without:
 Yucatán, Mexico, and the United States, 1880-
 1924," 505; Kazakova, "Zapadnaia Evropa v russkoi
 pis'mennosti, XV-XVI vekov," 145; Köhler, "Das
 Ende Preussens in französischer Sicht," 1015;
 Lensen, "Balance of Intrigue," 1306; Lipgens, "A
 History of European Integration," vol. 1: "1945-
 1947," 365; Paasivirta, "Finland and Europe," 414;
 Rohe, editor, "Die Westmächte und das Dritte
 Reich, 1933-1939," 1286; Smith, "Stitches in
 Time," 85; Stivers, "Supremacy and Oil: Iraq,
 Turkey, and the Anglo-American World Order,
 1918-1930," 1270; Taylor, "The Arab Balance of
 Power," 725; Wittner, "American Intervention in
 Greece, 1943-1949," 213
- "An Introduction to Nineteenth-Century Russian
 Slavophilism," vol. 3: "K. S. Aksakov," by Christoff,
 1026
- "Iran between Two Revolutions," by Abrahamian,
 1303
- The Iranian Revolution in Comparative Perspective*, by
 Keddie, 579-98
- "Ireland and the Irish," by Bottigheimer, 990
- "Ireland in Early Mediaeval Europe," edited by
 Whitelock *et al.* (E), 787
- Irick, Robert L., "Ch'ing Policy toward the Coolie
 Trade, 1847-1878," 732
- Iriye, Akira (R), 160
- Irons, Peter H., "The New Deal Lawyers," 496
- Irving, Robert Grant, "Indian Summer: Lutyens,
 Baker, and Imperial Delhi," 738
- Is There a New International History of the 1920s?*
 (Review Essay), by Jacobson, 617-45
- Isaac, Rhys, "The Transformation of Virginia, 1740-
 1790," 464
- Isern, Thomas D. (R), 758
- ISLAM: Bayat, "Mysticism and Dissent," 723; De Bruin,
 "Islam en nationalisme in door Japan bezet
 Indonesië, 1942-1945," 1312; Donner, "The Early
 Islamic Conquests," 440; Enayat, "Modern Islamic
 Political Thought," 724; Lewis, "The Muslim
 Discovery of Europe," 439; Lyons and Jackson,
 "Saladin," 721; Metcalf, "Islamic Revival in British
 India: Deoband, 1860-1900," 1050; Voll, "Islam,"
 1030
- "Islam," by Voll, 1030
- "Islam en nationalisme in door Japan bezet Indonesië,
 1942-1945," by De Bruin, 1312
- "Islamic Revival in British India: Deoband, 1860-
 1900," by Metcalf, 1050
- Ismael, Jacqueline S., "Kuwait: Social Change in
 Historical Perspective," 444
- Israel, Jonathan L., "The Dutch Republic and the
 Hispanic World, 1606-1661," 1003
- Isserman, Maurice, "Which Side Were You On? The
 American Communist Party during the Second
 World War," 776
- "Iz istorii krizisa russkoi burzhuazno-liberal'noi
 istoriografii," by Levandovskii, 1299
- "The Italian Crusades," by Housley, 378
- "Italian Fascism," by De Grand, 1019
- "Iuzhnaia Ukraina v period krizisa feodalizma, 1825-
 1860 gg.," by Druzhinina, 718
- Ivanova, L. V., "Formirovanie sovetskoi nauchnoi
 intelligentsii, 1917-1927 gg.," 720
- Iverson, Peter (R), 479
- Iverson, Peter, "Carlos Montezuma and the Changing
 World of American Indians," 1078
- "Jack in Port: Sailortowns of Eastern Canada," by
 Fingard, 1111
- Jackman, Jarrell C., and Carla M. Borden, editors,
 "The Muses Flee Hitler: Cultural Transfer and
 Adaptation, 1930-1945" (E), 1122
- Jackson, Carl T., "The Oriental Religions and
 American Thought: Nineteenth-Century
 Explorations," 193
- Jackson, D. E. P., and Malcolm Cameron Lyons,
 "Saladin: The Politics of the Holy War," 721
- Jackson, George D. (R), 79
- Jackson, Richard A. (R), 993
- "Jacksonian Politics and Community Conflict," by
 Watson, 181
- Jacob, Margaret C. (R), 389
- Jacob-Wendler, Gerhard, "Deutsche Elektroindustrie
 in Lateinamerika: Siemens und AEG, 1890-1914,"
 1352
- "The Jacobin Clubs in the French Revolution," by
 Kennedy, 404
- Jacobs, Dan N., "Borodin: Stalin's Man in China,"
 1029
- Jacobs, Ian, "Ranchero Revolt: The Mexican
 Revolution in Guerrero," 1351
- Jacobs, Sylvia M. (R), 1073
- Jacobs, Sylvia M., editor, "Black Americans and the
 Missionary Movement in Africa" (E), 791
- Jacobs, Travis Beal, "America and the Winter War,
 1939-1940," 210
- Jacobson, Jon, *Is There a New International History of the
 1920s?* (Review Essay), 617-45
- Jacoby, Russell, "Dialectic of Defeat: Contours of
 Western Marxism," 78
- Jacoway, Elizabeth, and David R. Colburn, editors,
 "Southern Businessmen and Desegregation," 1343
- Jaher, Frederic Cople, "The Urban Establishment:
 Upper Strata in Boston, New York, Charleston,
 Chicago, and Los Angeles," 167
- James, N. D. G., "A History of English Forestry," 108
- James I, 1262
- "James III," by Macdougall, 662
- "James F. Byrnes and the Origins of the Cold War,"
 edited by Clements (E), 235
- Jamieson, Perry D., and Grady McWhiney, "Attack
 and Die: Civil War Military Tactics and the
 Southern Heritage," 475
- Janics, Kálmán, "Czechoslovak Policy and the
 Hungarian Minority, 1945-1948," 714
- Janis, Ralph (R), 1088
- Janos, Andrew C., "The Politics of Backwardness in
 Hungary, 1825-1945," 433

- Jansson, Torkel, "Samhällsförändring och sammanslutningsformer: Det frivilliga föreningsväsendets uppkomst och spridning i Husby-Rekarne från omkring 1850 till 1930," 1279
- "Japan's Renaissance: The Politics of the Muromachi Bakufu," by Grossberg, 454
- Jarausch, Konrad H., "Students, Society, and Politics in Imperial Germany: The Rise of Academic Illiberalism," 702
- Jeanneney, Jean-Noël, and Monique Sauvage, editors, "Télévision, nouvelle mémoire: Les magazines de grand reportage, 1959–1968," 1275
- Jelavich, Barbara (R), 711
- Jencks, Harlan W., "From Muskets to Missiles: Politics and Professionalism in the Chinese Army, 1945–1981," 156
- Jenner, Edward, 676
- Jennings, Robert M., and Andrew P. Trout, "The Tontine from the Reign of Louis XIV to the French Revolutionary Era," 1272
- Jensen, Joan M., and Lois Scharf, editors, "Decades of Discontent: The Women's Movement, 1920–1940" (E), 1358
- Jenson, Carol E., "The Network of Control: State Supreme Courts and State Security Statutes, 1920–1970," 493
- "Jesus as Mother: Studies in the Spirituality of the High Middle Ages," by Bynum, 370
- "The Jews of Warsaw, 1939–1943," by Gutman, 716
- "The Jicarilla Apache Tribe," by Tiller, 1332
- Jirran, Raymond J. (C), 246
- Jochens, Jenny, M. (R), 694
- "Joe McCarthy and the Press," by Bayley, 1104
- Johansson, Sheila R. (R), 395
- "John A. Logan," by Jones, 1073
- "John Armstrong, Jr., 1758–1843," by Skeen, 470
- "John Dickinson: Conservative Revolutionary," by Flower, 1321
- "John Foster Dulles," by Preussen, 217
- "John Monash," by Serle, 1054
- "John Robinson and the English Separatist Tradition," by George, 1263
- "John Toland and the Deist Controversy," by Sullivan, 389
- "John X. Merriman," by Lewsen, 730
- Johns, Sheridan (R), 731
- Johnson, Arthur M., "The Challenge of Change: The Sun Oil Company, 1945–1977," 1343
- Johnson, Ban, 1095
- Johnson, Chalmers, "MITI and the Japanese Miracle: The Growth of Industrial Policy, 1925–1975," 158
- Johnson, James H., and Colin G. Pooley, editors, "The Structure of Nineteenth-Century Cities" (E), 787
- Johnson, John W., "American Legal Culture, 1908–1940," 203
- Johnson, Lyndon B., 502, 1346
- Johnson, Richard, *et al.*, editors, "Making Histories: Studies in History-Writing and Politics" (E), 1355
- Johnson, Richard R., "Adjustment to Empire: The New England Colonies, 1675–1715," 462
- Johnson, Robert Erwin (R), 778
- Jolivet, Jean, editor, "Abélard en son temps" (E), 233
- Jonas, Manfred (C), 1142
- Jonas, Manfred (R), 207
- Jonas, Manfred, and Robert V. Wells, editors, "New Opportunities in a New Nation: The Development of New York after the Revolution," 1061
- Jones, David, "Crime, Protest, Community, and Police in Nineteenth-Century Britain," 115
- Jones, Donald G. (R), 1323
- Jones, Dorothy V., "License for Empire: Colonialism by Treaty in Early America," 747
- Jones, Douglas Lamar, "Village and Seaport: Migration and Society in Eighteenth-Century Massachusetts," 174
- Jones, James P. (R), 1069
- Jones, James Pickett, "John A. Logan: Stalwart Republican from Illinois," 1073
- Jones, W. J. (R), 1262
- Jones, Wilbur Devereux (R), 1269
- Jónsson, Hannes, "Friends in Conflict: The Anglo-Icelandic Cod Wars and the Law of the Sea," 669
- "Joseph LeConte: Gentle Profit of Evolution," by Stephens, 1328
- Joseph, G. M., "Revolution from Without: Yucatán, Mexico, and the United States, 1880–1924," 505
- "Josephus in Galilee and Rome," by Cohen, 89
- Joughin, Jean T. (R), 126
- JOURNALISM AND THE MEDIA: Bayley, "Joe McCarthy and the Press," 1104; Czitrom, "Media and the American Mind," 745; Desmond, "Crisis and Conflict: World News Reporting between Two Wars," 1248; Jeanneney and Sauvage, editors, "Télévision, nouvelle mémoire: Les magazines de grand reportage, 1959–1968," 1275; Juergens, "News from the White House: The Presidential-Press Relationship in the Progressive Era," 765; Koss, "The Rise and Fall of the Political Press in Britain," 113; Manninen, "Vapaustaistelu, kansalaissota ja kapina: Taistelun luonne valkoisten sotapropagandassa vuonna 1918," 697; Nord, "Newspapers and New Politics: Midwestern Municipal Reform, 1890–1900," 1082; Rubin, "Press, Party, and Presidency," 746; Ruud, "Fighting Words: Imperial Censorship and the Russian Press, 1804–1906," 1298; Showalter, "Little Man, What Now? *Der Stürmer* in the Weimar Republic," 706; Stenbridge, "Parliament, the Press, and the Colonies, 1846–1880," 983; Udelson, "The Great Television Race," 492
- "Le journalisme d'Ancien Régime," edited by Rétat (E), 233
- Jowett, Garth, S. (R), 745
- JUDAISM: Angel, "La America: The Sephardic Experience in the United States," 1331; Best, "To Free a People: American Jewish Leaders and the Jewish Problem in Eastern Europe," 198; Birnbaum, "Staat und Synagoge, 1918–1938," 422; Blake, "Disraeli's Grand Tour," 981; Cohen, "English Zionists and British Jews," 682; Cohen, "The Friars and the Jews," 93; Edelstein, "An Unacknowledged Harmony: Philo-Semitism and the Survival of European Jewry," 972; Gruenwald, "Apocalyptic and Merkavah Mysticism," 371; Gutman, "The Jews of Warsaw, 1939–1943," 716; Halperin, "The Merkavah in Rabbinic Literature," 371; Katz, "Philo-Semitism and the Readmission of the Jews to England, 1603–1655," 672; Schultz, editor, "Mid-America's Promise: A Profile of Kansas City Jewry," 769; Toll, "The Making of an Ethnic Middle Class: Portland Jewry over Four Generations," 769; Wilson, "Ideology and Experience: Antisemitism in France at the Time of the Dreyfus Affair," 691; Yerushalmi, "Zakhor: Jewish History and Jewish Memory," 1239
- Judd, Denis, and Peter Slinn, "The Evolution of the Modern Commonwealth, 1902–80," 988
- "Judicial Reform in France before the Revolution of 1789," by Carev, 125
- Juergens, George (R), 746
- Juergens, George, "News from the White House: The Presidential-Press Relationship in the Progressive

- Era," 765
 "Les juifs en Roumanie, 1866–1919," by Iancu, 142
 "Julian and Hellenism," by Athanassiadi-Fowden, 90
 Jung, C. G., 1245
 Jupp, James, "The Radical Left in Britain, 1931–1941," 399
- Kagan, Donald, "The Peace of Nicias and the Sicilian Expedition," 86
 Kahr, William L., "Water and Power: The Conflict over Los Angeles' Water Supply in the Owens Valley," 201
 Kaiser, Daniel H. (R), 717
 "Kaiser- und Papstbriefe in den *Chronica majora* des Matthaeus Paris," by Hilpert, 102
 "Kaiser Wilhelm II," edited by Röhl and Sombart, 701
 Kaledin, Eugenia, "The Education of Mrs. Henry Adams," 196
 Kanman, William (R), 499
 Kamoche, Jidaph G., "Imperial Trusteeship and Political Evolution in Kenya, 1923–1963: A Study of the Official Views and the Road to Decolonization," 151
 "Kanazawa: A Seventeenth-Century Japanese Castle Town," by McClain, 1307
 Kanetsky, John W. (R), 114
 Kanner, Barbara Penny (R), 83
 Kanner, Barbara, editor, "The Women of England from Anglo-Saxon Times to the Present: Interpretive Bibliographical Essays," 1262
 Kaplan, Lawrence (R), 672
 Kaplan, Lawrence S., and Robert W. Clawson, editors, "NATO after Thirty Years" (E), 232
 Kaplan, Lawrence S., and Robert W. Clawson, editors, "The Warsaw Pact: Political Purpose and Military Means" (E), 233
 Kaplan, Steven L., and Dominick LaCapra, editors, "Modern European Intellectual History: Reappraisals and New Perspectives" (E), 787
 Kaplow, Jeffrey (R), 993
 Kappeler, Andreas, "Russlands erste Nationalitäten: Das Zarenreich und die Völker der Mittleren Wolga vom 16. bis 19. Jahrhundert," 718
 "Karajaan: Malay Political Culture on the Eve of Colonial Rule," by Milner, 458
 Kargon, Robert H., "The Rise of Robert Milikan: Portrait of a Life in American Science," 209
 Karpat, Kemal H. (R), 430
 Karsten, Peter (R), 1249
 Kasson, John F. (R), 468
 Kater, Michael H. (R), 668
 Katz, Barry, "Herbert Marcuse and the Art of Liberation: An Intellectual Biography," 963
 Katz, David S., "Philo-Semitism and the Readmission of the Jews to England, 1603–1655," 672
 Katz, Martin (R), 1026
 Katz, Michael B. (R), 481
 Katz, Michael B., *et al.*, "The Social Organization of Early Industrial Capitalism," 1109
 Kauffman, Christopher J., "Faith and Fraternalism: The History of the Knights of Columbus, 1882–1982," 775
 Kautsky, Karl, 1012
 "Kautskys Russisches Dossier," by Geyer, 1012
 Kay, Richard (R), 93
 Kazakova, N. A., "Zapadnaia Evropa v russkoi pis'mennosti, XV–XVI vekov: Iz istorii mezhdunarodnykh kul'turnykh svyazei Rossii," 145
 Kazhdan, Alexander, and Giles Constable, "People and Power in Byzantium: An Introduction to Modern Byzantine Studies," 379
 Kealey, Gregory S. (R), 219
 Kealey, Gregory S., and Bryan D. Palmer, "Dreaming of What Might Be: The Knights of Labor in Ontario, 1880–1900," 1349
 Keaney, John J. (R), 654
 Keddie, Nikki R., *The Iranian Revolution in Comparative Perspective*, 579–98
 Keddie, Nikki R., editor, "Religion and Politics in Iran: Shi'ism from Quietism to Revolution" (E), 1357
 Kedourie, Elie, and Sylvia G. Haim, editors, "Palestine and Israel in the Nineteenth and Twentieth Centuries" (E), 516
 Keen, Benjamin (R), 225
 Keep, John (C), 1138
 Kegley, Charles W., Jr., and Pat McGowan, editors, "Foreign Policy: USA/USSR" (E), 786
 Keller, Morton (R), 1063
 Kelley, Jonathan, and Herbert S. Klein, "Revolution and the Rebirth of Inequality: A Theory Applied to the National Revolution in Bolivia," 227
 Kelly, Aileen, "Mikhail Bakunin: A Study in the Psychology and Politics of Utopianism," 1243
 Kelly, George Armstrong, "Victims, Authority, and Terror: The Parallel Deaths of d'Orleans, Custine, Bailly, and Malesherbes," 125
 Kelly, Thomas (R), 966
 Kendle, John (R), 988
 Kennedy, Hugh (R), 149
 Kennedy, Kenneth A. R. (R), 649
 Kennedy, Michael L., "The Jacobin Clubs in the French Revolution: The First Years," 404
 Kerr, James E., "The Insular Cases: The Role of the Judiciary in American Expansionism," 187
 Kersaudy, François, "Churchill and De Gaulle," 107
 Kesner, Richard M. (R), 119
 Kesner, Richard M., "Economic Control and Colonial Development: Crown Colony Financial Management in the Age of Joseph Chamberlain," 117
 Kessell, John L. (R), 1340
 Kessler, Wolfgang, "Politik, Kultur und Gesellschaft in Kroatien und Slawonien in der ersten Hälfte des 19. Jahrhunderts: Historiographie und Grundlagen," 1020
 Keynes, Edward, "Undeclared War: Twilight Zone of Constitutional Power," 1056
 Keyvani, Mehdi, "Artisans and Guild Life in the Later Safavid Period: Contributions to the Social-Economic History of Persia," 1302
 Khánh, Huỳnh Kim, "Vietnamese Communism, 1925–1945," 459
 "Khazarian Hebrew Documents of the Tenth Century," by Golb and Pritsak, 1260
 "The Khilafat Movement," by Minault, 737
 Khoury, Philip S. (R), 725
 Kiernan, V. G., "From Conquest to Collapse: European Empires from 1815 to 1960," 651
 Kiernan, Victor G. (R), 983
 Kilander, Svenbjörn, "Censur och propaganda: Svensk informationspolitik under 1900-talets första decennier," 412
 Kim Il Sung, 160
 King, Lester S., "Medical Thinking: A Historical Preface," 654
 King, Martin Luther, Jr., 1108
 "The Kingdom of León-Castilla under Queen Urraca, 1109–1126," by Reilly, 376
 Kingdon, Robert M. (R), 698
 "The King's Coffers: Proprietors of the Spanish

- Florida Treasury," by Bushnell, 223
 "The King's Debts: Finance and Politics in France, 1589–1661," by Bonney, 123
 Kirkman, James (R), 727
 Kirschbaum, Stanislaw (R), 714
 Kirshner, Julius (R), 1258
 Király, Béla K., and János M. Bak, editors, "From Hunyadi to Rákóczi: War and Society in Late Medieval and Early Modern Hungary" (E), 1123
 "The Kit Carson Campaign: The Last Great Navajo War," by Trafzer, 479
 Kitchen, Martin, "The Coming of Austrian Fascism," 135
 Klarén, Peter F. (R), 1118
 Klein, Herbert S. (R), 361
 Klein, Herbert S., "Bolivia: The Evolution of a Multi-Ethnic Society," 510
 Klein, Herbert S., *The Integration of Italian Immigrants into the United States and Argentina: A Comparative Analysis* (Forum), 306–29
 Klein, Herbert S., *Reply to Comments on Klein, The Integration of Italian Immigrants into the United States and Argentina* (Forum), 343–46
 Klein, Herbert S., and Jonathan Kelley, "Revolution and the Rebirth of Inequality: A Theory Applied to the National Revolution in Bolivia," 227
 Klein, Herbert S., Stanley L. Engerman, and Manuel Moreno Fraginals, *The Level and Structure of Slave Prices on Cuban Plantations in the Mid-Nineteenth Century: Some Comparative Perspectives*, 1201–18
 Klein, Milton M. (R), 174
 Kleppner, Paul, *et al.*, "The Evolution of American Electoral Systems," 168
 Kling, Blair B. (R), 737
 Klinkhamer, Marie C. (R), 187
 Knapp, Vincent J. (R), 1021
 Knecht, R. J., "Francis I," 122
 Knight, Thomas J. (R), 653, 1353
 "Knights of the Road: A Hobo History," by Bruns, 494
 Knox, MacGregor, "Mussolini Unleashed, 1939–1941: Politics and Strategy in Fascist Italy's Last War," 138
 "Koblenz im 18. Jahrhundert," by François, 700
 Kocka, Jürgen (R), 1281
 Koehler, Lyle (R), 750
 Koenker, Ernest B. (R), 970
 Kogan, Norman, "A Political History of Postwar Italy: From the Old to the New Center-Left," 138
 Koh, B. C. (R), 160
 Kohl, James V. (R), 510
 Köhler, Henning, "Das Ende Preussens in französischer Sicht," 1015
 Köhler, Peter A., *et al.*, "The Evolution of Social Insurance, 1881–1981: Studies of Germany, France, Great Britain, Austria, and Switzerland" (E), 515
 Kolb, Robert (R), 1006
 "Konfessionskonflikt und Staatsbildung," by Schilling, 415
 "Könige, Stadt und Kapital: Aufsätze zur Wirtschafts- und Sozialgeschichte des Mittelalters," by Peyer (E), 1121
 "Korea and Two Regimes," by Yang, 160
 Kornilov, Aleksandr Aleksandrovich, 1299
 Koschmann, J. Victor, and Tetsuo Najita, editors, "Conflict in Modern Japanese History: The Neglected Tradition," 1047
 Koss, Stephen, "The Rise and Fall of the Political Press in Britain: The Nineteenth Century," 113
 Kottje, Raymund, and Harald Zimmermann, editors, "Hrabanus Maurus: Lehrer, Abt und Bischof," 969
 Kousser, J. Morgan, and James M. McPherson, editors, "Region, Race, and Reconstruction: Essays in Honor of C. Vann Woodward," 476
 Kovacs, Martin L. (R), 220
 Kraditor, Aileen S., "The Radical Persuasion, 1890–1917: Aspects of the Intellectual History and the Historiography of Three American Radical Organizations," 491
 Kramer, Steven Philip (R), 692, 1275
 Krausnick, Helmut, and Hans-Heinrich Wilhelm, "Die Truppe des Weltanschauungskrieges: Die Einsatzgruppen der Sicherheitspolizei und des SD, 1938–1942," Part 1, "Die Einsatzgruppen vom Anschluss Österreichs bis zum Feldzug gegen die Sowjetunion: Entwicklung und Verhältnis zur Wehrmacht"; Part 2, "Die Einsatzgruppe A der Sicherheitspolizei und des SD, 1941/2: Eine exemplarische Studie," 422
 Krausz, Michael, and Jack W. Meiland, editors, "Relativism: Cognitive and Moral" (E), 231
 Krentz, Peter, "The Thirty at Athens," 654
 Kreuger, Thomas A. (R), 482
 "Krieg—Frieden—Abrüstung: Die Haltung der europäischen und amerikanischen Kirchen zur Frage der deutschen Kriegsschuld, 1914–1933," by Besier, 703
 Kroker, James P. (R), 1020
 Krumbholz, Joachim, "Die Elementarbildung in Russland bis zum Jahre 1864: Ein Beitrag zur Entstehung des Volksschulstatuts vom 14. Juli 1864," 1027
 Kruskal, William H., editor, "The Social Sciences: Their Nature and Uses" (E), 232
 Kryzanek, Michael J., and Howard J. Wiarda, "The Dominican Republic: A Caribbean Crucible," 503
 Kubicek, Robert (R), 1041
 Kubler, George, "Building the Escorial," 410
 Kuehn, Thomas, "Emancipation in Late Medieval Florence," 378
 Kuliscioff, Anna, 427
 "Kuninkaan 'perintöä ja omaa' (arv och eget)," by Lantinen, 696
 Kunoff, Hugo, "The Foundations of the German Academic Library," 1283
 Kunz, Hans B., "Weltrevolution und Völkerbund: Die schweizerische Aussenpolitik unter dem Eindruck der bolschewistischen Bedrohung, 1918–1923," 136
 Kuropiatnik, G. P., "Rossiia i SShA: Ekonomicheskie, kul'turnye i diplomaticheskie sviazi, 1867–1881," 1300
 Kushma, John J., and Stephen E. Maizlish, editors, "Essays on American Antebellum Politics, 1840–1860," 1064
 Kutler, Stanley I., "The American Inquisition: Justice and Injustice in the Cold War," 1104
 "Kuwait," by Ismael, 444
 La Follette, Philip F., 1097
 LABOR AND LABOR HISTORY: Berlin and Gutman, *Natives and Immigrants, Free Men and Slaves: Urban Workingmen in the Antebellum South, 1175–1200*; Blanchard, "The Origins of the Peruvian Labor Movement, 1883–1919," 1118; Brown, "The English Labour Movement, 1700–1951," 394; Byrkit, "Forging the Copper Collar: Arizona's Labor-Management War of 1901–1921," 1090; Clark, "The Miners' Fight for Democracy," 1096; Corbin, "Life, Work, and Rebellion in the Coal Fields," 1096; Cornell, "Sundsvallsdistriktets sågverksarbetare, 1860–1890," 412; Craven, "'An Impartial Umpire': Industrial Relations and the

- Canadian State," 219; Englisch, "Baunkohlenbergbau und Arbeiterbewegung," 1021; Foster, "American Labor in the Southwest," 200; Harris, "The Harder We Run: Black Workers since the Civil War," 768; Kealey and Palmer, "Dreaming of What Might Be: Knights of Labor in Ontario, 1880–1900," 1349; Keyvani, "Artisans and Guild Life in the Later Safavid Period," 1302; Malcolmson, "Life and Labour in England, 1700–1780," 1264; Richardson, "Chinese Mine Labour in the Transvaal," 1041; Saunders, "Workers in Bondage: The Origins and Bases of Unfree Labour in Queensland," 741; Seaton, "Catholics and Radicals: The Association of Catholic Trade Unionists and the American Labor Movement," 208; Shergold, "Working-Class Life: The 'American Standard' in Comparative Perspective, 1899–1913," 1089; Triska and Gati, editors, "Blue-Collar Workers in Eastern Europe," 140
- Laboratory for the Oral History of Slavery: The Island of Lamu on the Kenya Coast*, by Curtin, 858–82
- LaCapra, Dominick (C), 805
- LaCapra, Dominick (R), 648
- LaCapra, Dominick, "Madame Bovary on Trial," 129
- LaCapra, Dominick, and Steven L. Kaplan, editors, "Modern European Intellectual History: Reappraisals and New Perspectives" (E), 787
- Lachiver, Marcel, "Vin, vigne, et vigneron en région parisienne du XVII^e au XIX^e siècle," 995
- Lackner, Bede Karl, and Gary D. Stark, editors, "Essays on Culture and Society in Modern Germany," 1282
- Ladd, Doris M. (R), 507
- "Ladies of the Leisure Class: The Bourgeoises of Northern France in the Nineteenth Century," by Smith, 405
- Lael, Richard L. "The Yamashita Precedent: War Crimes and Command Responsibility," 1249
- Lafore, Laurence (R), 1247
- Lajous, Alejandra, "Los orígenes del Partido Único en México," 1116
- Lake, Peter, "Moderate Puritans and the Elizabethan Church," 670
- Lalbahai, Kasturbhai, 1051
- Lamar, Howard, and Leonard Thompson, editors, "The Frontier in History: North America and Southern Africa Compared," 84
- Lampe, John R., and Marvin R. Jackson, "Balkan Economic History, 1550–1950: From Imperial Borderlands to Developing Nations," 139
- Lamphear, John (R), 1038
- "Lancastrian Englishmen," by Reeves, 98
- "Land and Revolution in Iran, 1960–1980," by Hooglund, 1033
- "Land and Society in Early Scotland," by Dodgshon, 376
- Land, Aubrey C. (R), 463
- Lander, Ernest McPherson, Jr., "The Calhoun Family and Thomas Green Clemson: The Decline of a Southern Patriarchy," 1323
- Lander, J. R. (R), 661
- "Der landesherrliche Hofrat unter Herzog und Kurfürst Maximilian I. von Bayern, 1598–1651," by Heydenreuter, 1007
- Landi, Lando, "L'Inghilterra il pensiero politico di Montesquieu," 381
- Langewiesche, Dieter, and Klaus Schönhoven, editors, "Arbeiter in Deutschland: Studien zur Lebensweise der Arbeiterschaft im Zeitalter der Industrialisierung" (E), 234
- Läntinen, Aarre, "Kuninkaan 'perintöä ja omaa' (arv och eget): Kameraalihistoriallinen tutkimus Kustaa Vaasan maaomaisuudesta Suomessa vuosina, 1531–1560," 696
- Lapidus, Ira M. (R), 650
- Large, Stephen S. (C), 525
- Laslett, J. H. M. (C), 529
- "Latin American Integration," by Bawa, 224
- "The Latin Church in the Crusader States," by Hamilton, 96
- Launius, Roger D. (R), 756
- Laurent, Pierre-Henri (R), 365
- "Lavoro e socialismo: Abbozzo di una storia della concezione socialista del lavoro," by Ghibaudi, 963
- "The Law of Treason and Treason Trials in Later Medieval France," by Cuttler, 100
- Lawson, Michael L., "Damned Indians: The Pick-Sloan Plan and the Missouri River Sioux, 1944–1980," 500
- "Lawyers and Politics in the Arab World, 1880–1960," by Reid, 150
- "Lay Culture, Learned Culture: Books and Social Change in Strasbourg, 1480–1599," by Chrisman, 1005
- Lazerson, Marvin (R), 1055
- Lazerson, Marvin, and W. Norton Grubb, "Broken Promises: How Americans Fail Their Children," 762
- Lear, Linda J., "Harold L. Ickes: The Aggressive Progressive, 1874–1933," 206
- Lears, Jackson (R), 165
- Leavitt, Judith Walzer (R), 764
- Leavitt, Judith Walzer, "The Healthiest City: Milwaukee and the Politics of Health Reform," 488
- LeConte, Joseph, 1328
- Le Cordeur, Basil A., "The Politics of Eastern Cape Separatism, 1820–1854," 152
- Lee, Arthur, 748
- Lee, J. M., and Martin Petter, "The Colonial Office, War, and Development Policy: Organisation and the Planning of a Metropolitan Initiative, 1939–1945," 119
- Lee, Loyd E. (R), 129
- Lee, R. Alton, "Dwight D. Eisenhower: Soldier and Statesman," 215
- Leed, Eric J. (R), 382
- Leff, Nathaniel H., "Underdevelopment and Development in Brazil," vol. 1: "Economic Structure and Change, 1822–1947"; vol. 2: "Reassessing the Obstacles to Economic Development," 782
- Left Hand, Chief of the Arapahoes, 479
- LEGAL HISTORY AND THE LAW: Alekseev, "Pskovskaia Sudnaia gramota i ee vremia," 145; Basch, "In the Eyes of the Law: Women, Marriage, and Property in Nineteenth-Century America," 485; Carey, "Judicial Reform in France before the Revolution of 1789," 125; Ch'en, "The Formation of the Early Meiji Legal Order," 733; Cuttler, "The Law of Treason and Treason Trials in Later Medieval France," 100; Dewindt and Dewindt, "Royal Justice and the Medieval English Countryside," 373; Honoré, "Emperors and Lawyers," 658; Hyman and Wiecek, "Equal Justice under Law: Constitutional Development, 1835–1875," 469; Irons, "The New Deal Lawyers," 496; Jenson, "The Network of Control: State Supreme Courts and State Security Statutes, 1920–1970," 493; Johnson, "American Legal Culture, 1908–1940," 203; Jónsson, "Friends in Conflict: The Anglo-Icelandic Cod Wars," 669; Kerr, "The Insular Cases: The Role of the Judiciary in American Expansionism," 187; Kuehn, "Emancipation in Late Medieval Florence," 378; Makdisi, "The Rise of Colleges:

- Institutions of Learning in Islam and the West," 442; Miller, "Toward Increased Judicial Activism," 1107; Reid, "The Defiance of the Law: The Standing-Army Controversy, the Two Constitutions, and the Coming of the American Revolution," 177; Reid, "Lawyers and Politics in the Arab World, 1880–1960," 150; Rogoziński, "Power, Caste, and Law," 663; Shaw, "Legal Norms in a Confucian State," 455; Summers, "Instrumentalism and American Legal Theory," 1335; Watson, "The Making of the Civil Law," 83; White, "Earl Warren," 777
- "Legal Norms in a Confucian State," by Shaw, 455
- Lehmann, Jean-Pierre, "The Roots of Modern Japan," 1047
- "Leisure and Urbanism in Nineteenth-Century Nice," by Haug, 997
- Lemahieu, D. L. (R), 110
- Lemass, Seán, 1272
- "Lend-Lease, Loans, and the Coming of the Cold War," by Martel, 211
- "Lenin Lives! The Lenin Cult in Soviet Russia," by Tumarkin, 1301
- Lenin, Nikolai, 77
- Lennon, Colm, "Richard Stanihurst: The Dubliner, 1547–1618; A Biography with a Stanihurst Text on Ireland's Past," 991
- Lennon, Thomas M., *et al.*, editors, "Problems of Cartesianism" (E), 785
- Lensen, George Alexander, "Balance of Intrigue: International Rivalry in Korea and Manchuria, 1884–1899," 1306
- "Leon Trotsky's Theory of Revolution," by Molyneux, 79
- Leonard, Charlene M. (R), 997
- Leopold, Joan, "Culture in Comparative and Evolutionary Perspective: E. B. Tylor and the Making of *Primitive Culture*," 649
- Lerda, Valeria Gennaro, "Il Populismo americano: Movimenti radicali di protesta agraria nella seconda metà del '800," 1329
- Lerner, Robert E. (R), 1255
- Leroy, Gérard, "Péguy entre l'ordre et la révolution," 407
- Le Roy Ladurie, Emmanuel, and Joseph Goy, "Tithe and Agrarian History from the Fourteenth to the Nineteenth Centuries: An Essay in Comparative History," 1240
- Lerski, George J. (R), 1103
- Lescure, Michel, "Les banques, l'état et le marché immobilier en France à l'époque contemporaine, 1820–1940," 690
- "Let the Trumpet Sound: The Life of Martin Luther King, Jr.," by Oates, 1108
- Leutze, James R. (R), 211
- Levandovskii, A. A., "Iz istorii krizisa russkoi burzhuažno-liberal'noi istoriografii: A. A. Kornilov," 1299
- The Level and Structure of Slave Prices on Cuban Plantations in the Mid-Nineteenth Century: Some Comparative Perspectives*, by Friginals, Klein, and Engerman, 1201–18
- Levin, Daniel H. (R), 778
- Levin, N. Gordon, Jr. (R), 765
- Levine, Daniel (R), 492
- Levine, Joseph M. (R), 971
- Levine, Robert M. (R), 228
- Levitt, James H., "For Want of Trade: Shipping and the New Jersey Ports, 1680–1783," 463
- Levy, Richard S. (R), 422
- Lewandowski, Susan, "Migration and Ethnicity in Urban India: Kerala Migrants in the City of Madras, 1870–1970," 456
- Lewenhak, Sheila (R), 986
- Lewin, Ronald, "The American Magic: Codes, Ciphers, and the Defeat of Japan," 211
- Lewis, Andrew W. (R), 101
- Lewis, Andrew W., "Royal Succession in Capetian France: Studies on Familial Order and the State," 99
- Lewis, Archibald R. (R), 370
- Lewis, Bernard, "The Muslim Discovery of Europe," 439
- "Lewis Morris, 1671–1746," by Sheridan, 174
- Lewis, Richard D. (R), 437
- Lewsen, Phyllis, "John X. Merriman: Paradoxical South African Statesman," 730
- LIBERALISM: May, "From New Deal to New Economics: The American Liberal Response to the Recession of 1937," 495
- "Liberalism in an Illiberal Age: New Culture Liberals in Republican China, 1919–1937," by Lubot, 1045
- "Liberian Diplomacy in Africa," by Holloway, 1036
- "Liberty's Daughters: The Revolutionary Experience of American Women," by Norton, 750
- LIBRARIES AND ARCHIVES: Ambrosini, "Paesi e mari ignoti: America e colonialismo europeo nella cultura veneziana, secoli XVI–XVII," 1017; Bratzel and Rout, *Once Again: Pearl Harbor, Microdots, and J. Edgar Hoover* (Research Note), 953–60; Kunoff, "The Foundations of the German Academic Library," 1283
- Licate, Jack A., "Creation of a Mexican Landscape: Territorial Organization and Settlement in the Eastern Puebla Basin, 1520–1605," 779
- "License for Empire: Colonialism by Treaty in Early America," by Jones, 747
- Lida, Clara E. (R), 1276
- Lidtkje, Vernon (R), 1285
- Liebel-Weckowicz, Helen (R), 1284
- Liebknecht, Wilhelm, 1012
- Liehr, Reinhard, "Sozialgeschichte spanischer Adelskorporationen: Die Maestranzas de Caballería, 1670–1808," 1001
- "The Life and Art of James Barry," by Pressly, 980
- "Life and Labour in England, 1700–1780," by Malcolmson, 1264
- "The Life and Travels of John Bartram," by Berkeley and Berkeley, 175
- "Life Histories and Psychobiography," by Runyan, 1238
- "Life, Work, and Rebellion in the Coal Fields," by Corbin, 1096
- Lilie, Ralph-Johannes, "Byzanz und die Kreuzfahrerstaaten: Studien zur Politik des Byzantinischen Reiches gegenüber den Staaten der Kreuzfahrer in Syrien und Palästina bis zum vierten Kreuzzug, 1096–1204," 95
- Limbaugh, Ronald H., "Rocky Mountain Carpetbaggers: Idaho's Territorial Governors, 1863–1890," 757
- "The Limits of State Autonomy: Post-Revolutionary Mexico," by Hamilton, 780
- "The Limits to Capital," by Harvey, 1243
- Lincoln, Abraham, 184, 754, 1070
- Lincoln, W. Bruce (R), 1298
- Lincoln, W. Bruce, "In the Vanguard of Reform: Russia's Enlightened Bureaucrats, 1825–1861," 1298
- "Lincoln and Black Freedom," by Cox, 473
- "Lincoln's Quest for Union," by Stozier, 1070
- Lindemann, Albert S. (R), 382

- Lindley, Keith, "Fenland Riots and the English Revolution," 388
 Link, Arthur, *et al.*, editors, "The Papers of Woodrow Wilson," vol. 36: "January 27–May 8, 1916"; vol. 37: "May 9–August 7, 1916"; vol. 38: "August 7–November 19, 1916," 1091
 Link, Arthur S., editor, "Woodrow Wilson and a Revolutionary World, 1913–1921," 765
 Linnemann, Russell J., editor, "Alain Locke: Reflections on a Modern Renaissance Man" (E), 1124
 "The Lion of Freedom: Feargus O'Connor and the Chartist Movement, 1832–1842," by Epstein, 393
 Lippens, Walter, "A History of European Integration," vol. 1: "1945–1947: The Formation of the European Unity Movement," 365
 Lipow, Arthur, "Authoritarian Socialism in America: Edward Bellamy and the Nationalist Movement," 194
 Lippy, Charles H., "Seasonable Revolutionary: The Mind of Charles Chauncy," 749
 Lipsitz, George (C), 527
 Liss, Peggy K. (R), 1114
 "The Literary Underground of the Old Regime," by Darnton, 687
 "The Literate Revolution in Greece and Its Cultural Consequences," by Havelock (E), 516
 LITERATURE: Darnton, "The Literary Underground of the Old Regime," 687; Fender, "Plotting the Golden West: American Literature and the Rhetoric of the California Trail," 183; Grassl, "Sozialökonomische Vorstellungen in der kaiserzeitlichen griechischen Literatur," 1253; LaCapra, "Madame Bovary on Trial," 129; Maguire, "Art and Eloquence in Byzantium," 380; Nieuwenhuys, "Mirror of the Indies," 1052; Pickowicz, "Marxist Literary Thought in China," 155; Pike, "German Writers in Soviet Exile, 1933–1945," 133; Wagar, "Terminal Visions," 962
 "Little Italies in North America," edited by Harney and Scarpaci, 191
 "Little Man, What Now? *Der Stürmer* in the Weimar Republic," by Showalter, 706
 Liu, Tai (R), 1263
 "Lives of Their Own: Blacks, Italians, and Poles in Pittsburgh, 1900–1960," by Bodnar *et al.*, 1089
 Livingston, James C. (R), 111
 "The Livonian Crusade," by Urban, 96
 Lloyd, Henry Demarest, 1330
 Lloyd, T. H., "Alien Merchants in England in the High Middle Ages," 661
 Lloyd George, David, 118
 Lloyd-Jones, Hugh, *et al.*, editors, "History and Imagination: Essays in Honor of H. R. Trevor-Roper" (E), 231
 LOCAL HISTORY: Aycoberry, "Cologne entre Napoléon et Bismarck," 418; Booth, "The Financial Administration of the Lordship and County of Chester, 98; Carls-Maire, "La Ville Libre de Dantzig en crise ouverte 24.10.1938–1.9.1939," 1022; Fisher, "Custom, Work, and Market Capitalism: The Forest of Dean Colliers, 1788–1888," 394; Hanson, "The Civilian Population and the Warsaw Uprising of 1944," 1022; Hast, "Loyalism in Revolutionary Virginia," 177; Schek, "Zwischen Weltkrieg und Revolution," 132
 Lockard, Craig A. (R), 1053
 Locke, Alain, 1124
 Locke, Don, "A Fantasy of Reason: The Life and Thought of William Godwin," 392
 Lockhart, James (R), 1113
 Lockmann, Ronald F., "Guarding the Forests of Southern California: Evolving Attitudes Toward Conservation of Watershed, Woodlands, and Wilderness," 202
 Loesch, John R., "The Divine Community: Trinity, Church, and Ethics in Reformation Theologies," 380
 Logan, John A., 1073
 Lomask, Milton, "Aaron Burr: The Conspiracy and Years of Exile, 1805–1836," 1062
 Lomax, Derek W. (R), 376
 Lombardi, John V., "Venezuela: The Search for Order, the Dream of Progress," 782
 "London Chartism, 1838–1848," by Goodway, 112
 Long, E. B., "The Saints and the Union: Utah Territory during the Civil War," 756
 Long, Huey, 494
 "Long Memory: The Black Experience in America," by Berry and Blassingame, 1342
 Lorcin, Marie-Thérèse, "Vivre et mourir en Lyonnais: A la fin du Moyen Âge," 101
 "Lord Swinton," by Cross, 1271
 Lorenz, Ina Susanne, "Eugen Richter: Der entschiedene Liberalismus in wilhelminischer Zeit, 1871 bis 1906," 418
 "The Lost Sisterhood: Prostitution in America, 1900–1918," by Rosen, 1084
 Lough, John, "The *Philosophes* and Post-Revolutionary France," 402
 Louis, Wm. Roger, and Prosser Gifford, editors, "The Transfer of Power in Africa: Decolonization, 1940–1960" (E), 790
 "Love of Order: South Carolina's First Secession Crisis," by Barnwell, 473
 Lovett, Clara M. (R), 710
 Lowe, Peter (R), 1049
 Löwy, Michael, "The Politics of Combined and Uneven Development: The Theory of Permanent Revolution," 76
 "Loyalism in Revolutionary Virginia," by Hast, 177
 Luard, Evan, "A History of the United Nations," vol. 1: "The Years of Western Domination, 1945–1955," 366
 Lubot, Eugene, "Liberalism in an Illiberal Age: New Culture Liberals in Republican China, 1919–1937," 1045
 Luft, David S. (R), 426
 "Lugard and the Abeokuta Uprising," by Gailey, 1036
 Lui, Adam Yuen-Chung, "The Hanlin Academy: Training Ground for the Ambitious, 1644–1850," 1041
 Lukács, Gyorgy, 1245
 Lukács, Lajos, "The Vatican and Hungary, 1846–1878: Reports and Correspondence on Hungary of the Apostolic Nuncios in Vienna," 1295
 Lukas, Richard C., "Bitter Legacy: Polish-American Relations in the Wake of World War II," 1103
 "Lumberjacks and Legislators," by Robbins, 1087
 Lundbäck, Britt-Marie, "En industri kommer till stan: Hudiksvall och trävaruindustrin, 1855–1876," 695
 Lunn, Eugene, "Marxism and Modernism: An Historical Study of Lukács, Brecht, Benjamin, and Adorno," 1245
 Lustig, R. Jeffrey, "Corporate Liberalism: The Origins of Modern American Political Theory, 1890–1920," 1083
 Lutvins, Sir Edwin, 738
 Lutz, Jessie G. (R), 452
 Lynch, Joseph H. (R), 1257
 Lyon, Bryce (R), 372
 Lyons, Malcolm Cameron, and D. E. P. Jackson,

- "Saladin: The Politics of the Holy War," 721
 Lyons, Paul, "Philadelphia Communists, 1936–1956," 1101
 Lytle, Guy Fitch, and Stephen Orgel, editors, "Patronage in the Renaissance" (E), 789
- Maas, Clifford W., "The German Community in Renaissance Rome, 1378–1523," 137
 Mably, Gabriel Bonnot de, 402
 Mabry, Donald J. (R), 1116
 McCarran, Pat, 1099
 "McCarthy in Wisconsin," by O'Brien, 217
 McCarthy, Joseph, 1104
 McCarthy, Kathleen D. (R), 1081
 McCarthy, Kathleen D., "Noblesse Oblige: Charity and Cultural Philanthropy in Chicago, 1849–1929," 483
 McClain, James L., "Kanazawa: A Seventeenth-Century Japanese Castle Town," 1307
 McClellan, Woodford (R), 78
 McCormick, Richard P. (R), 460
 McCormick, Richard P., "The Presidential Game: The Origins of American Presidential Politics," 461
 McCoy, Alfred W., and Ed. C. De Jesus, editors, "Philippine Social History: Global Trade and Local Transformations," 1310
 McCoy, Donald R. (R), 1344
 McCrary, Peyton (R), 186
 McCulloch, Samuel Clyde (R), 163
 McDonald, Archie P. (R), 1322
 McDonald, Michael J., and John Muldowny, "TVA and the Dispossessed: The Resettlement of Population in the Norris Dam Area," 1341
 MacDonald, William W., "The Making of an English Revolutionary: The Early Parliamentary Career of John Pym," 108
 MacDougall, Hugh A., "Racial Myth in English History: Trojans, Teutons, and Anglo-Saxons," 679
 Macdougall, Norman, "James III: A Political Study," 662
 McDowell, John Patrick, "The Social Gospel in the South: The Woman's Home Mission Movement in the Methodist Episcopal Church, South, 1886–1939," 1084
 McDowell, R. B., and D. A. Webb, "Trinity College, Dublin, 1592–1952: An Academic History," 684
 McGiffert, Michael, *God's Controversy with Jacobean England*, 1151–74
 McGovern, James R., "Anatomy of a Lynching: The Killing of Claude Neal," 772
 McGowan, Bruce, "Economic Life in Ottoman Europe: Taxation, Trade, and the Struggle for Land, 1600–1800," 430
 McGowan, Pat, and Charles W. Kegley, Jr., editors, "Foreign Policy: USA/USSR" (E), 786
 McGregor, Alexander Campbell, "Counting Sheep: From Open Range to Agribusiness on the Columbia Plateau," 1087
 McGuire, Brian Patrick, "The Cistercians in Denmark: Their Attitudes, Roles, and Functions in Medieval Society," 664
 Macías, Anna, "Against All Odds: The Feminist Movement in Mexico to 1940," 507
 McKale, Donald M. (R), 704
 McKay, John P. (R), 438
 McKelvey, James L. (R), 674
 McKendrick, Neil, *et al.*, "The Birth of a Consumer Society: The Commercialization of Eighteenth-Century England," 977
 MacKenzie, Jeanne, and Norman MacKenzie, editors, "The Diary of Beatrice Webb," vol. 1: "Glitter Around and Darkness Within," 1873–1892," 984
 MacKenzie, Norman, and Jeanne MacKenzie, editors, "The Diary of Beatrice Webb," vol. 1: "Glitter Around and Darkness Within," 1873–1892," 984
 McLane, John R. (R), 456
 McLean, Malcolm D., "Papers concerning Robertson's Colony in Texas," vol. 9: "October 1834 through March 20, 1835: Sarahlville de Viesca," 1322
 Maclear, J. F. (R), 1317
 McLellan, David S. (R), 217
 McLellan, David, editor, "Marx: The First Hundred Years" (E), 1356
 McLoughlin, William G. (R), 484
 McLuhan, Marshall, 745
 McMahon, Robert J., "Colonialism and Cold War: The United States and the Struggle for Indonesian Independence, 1945–49," 498
 McManners, John, "Death and the Enlightenment: Changing Attitudes to Death among Christians and Unbelievers in Eighteenth-Century France," 124
 McMurray, Linda O., "George Washington Carver: Scientist and Symbol," 197
 McNeal, Robert H. (R), 1301
 McNeill, William H., "The Pursuit of Power: Technology, Armed Force, and Society since A.D. 1000," 1239
 McPherson, James M., and J. Morgan Kousser, editors, "Region, Race, and Reconstruction: Essays in Honor of C. Vann Woodward," 476
 McWhiney, Grady, and Perry D. Jamieson, "Attack and Die: Civil War Military Tactics and the Southern Heritage," 475
 "Madame Bovary on Trial," by LaCapra, 129
 Madden, Edward H., and James E. Hamilton, "Freedom and Grace: The Life of Asa Mahan," 1323
 Madden, Frederick, and D. K. Fieldhouse, editors, "Oxford and the Ideal of Commonwealth: Essays Presented to Sir Edgar Williams" (E), 515
 Maddux, Thomas R. (R), 210
 Madison, James H., "The History of Indiana," vol. 5: "Indiana through Tradition and Change: A History of the Hoosier State and Its People, 1920–1945," 773
 Madison, K. G. (R), 98
 Maguire, Henry, "Art and Eloquence in Byzantium," 380
 Mahan, Asa, 1323
 Maier, Charles S. (R), 1246
 Main, Gloria L. (R), 748
 Main, Gloria L., "Tobacco Colony: Life in Early Maryland, 1650–1720," 1318
 Main, Jackson T. (R), 178
 Maizlish, Stephen E., and John J. Kushma, editors, "Essays on American Antebellum Politics, 1840–1860," 1064
 Major, J. Russell (C), 245
 Makdisi, George, "The Rise of Colleges: Institutions of Learning in Islam and the West," 442
 "Die Makedonische Frage," by Adanir, 1019
 Mäkelä, Anneli (R), 696
 "Making Histories: Studies in History-Writing and Politics," edited by Johnson *et al.* (E), 1355
 "The Making of the Civil Law," by Watson, 83
 "The Making of an English Revolutionary: The Early Parliamentary Career of John Pym," by MacDonald, 108
 "The Making of an Ethnic Middle Class: Portland Jewry over Four Generations," by Toll, 769
 "The Making of the Industrial Landscape," by Trinder, 1264

- "The Making of Modern British Politics, 1867–1939," by Pugh, 678
- "The Making of Roman Italy," by Salmon, 655
- "The Making of Technological Man," by Weiss, 127
- "Making Sense of Self: Medical Advice Literature in Late Nineteenth-Century America," by Fellman, 194
- Malcolmson, Robert W., "Life and Labour in England, 1700–1780," 1264
- Malebranche, Nicholas, 75
- Malino, Frances (R), 405
- Malloy, James M. (R), 227
- Mamatey, Victor S. (R), 712
- "Managing International Crises," edited by Frei (E), 1120
- "The Mandarin-Capitalists from Nanyang," by Godley, 1043
- Mandle, W. F., and G. Osborne, editors, "New History: Studying Australia Today" (E), 516
- "Manitou and Providence: Indians, Europeans, and the Making of New England," by Salisbury, 746
- Mann, Ralph, "After the Gold Rush: Society in Grass Valley and Nevada City, California, 1849–1870," 1326
- Mannheim, Karl, 77
- Manninen, Turo, "Vapaustaistelu, kansalaissota ja kapina: Taistelun luonne valkoisten sotapropagandassa vuonna 1918," 697
- "Manning Clark and Australian History, 1915–1963," by Holt, 742
- Manning, Patrick, *Contours of Slavery and Social Change in Africa*, 835–57
- Manning, Patrick, "Slavery, Colonialism, and Economic Growth in Dahomey, 1640–1960," 727
- Manning, Roberta Thompson, "The Crisis of the Old Order in Russia: Gentry and Government," 1028
- Mansingh, Surjit (R), 162
- Mao Tse-Tung, 451
- Maravall, José Antonio, "Utopia y reformismo en la España de los Austrias," 1000
- "The March to the Marne: The French Army, 1871–1914," by Porch, 407
- Marcus, Ivan G. (R), 371
- Marcuse, Herbert, 963
- Markovits, Andrei S., and Frank S. Sysvn, editors, "Nationbuilding and the Politics of Nationalism: Essays on Austrian Galicia" (E), 1122
- Marling, Karal Ann, "Wall-to-Wall America: A Cultural History of Post-Office Murals in the Great Depression," 775
- "Marriage and Society: Studies in the Social History of Marriage," edited by Outhwaite (E), 514
- "Married Priests and the Reforming Papacy," by Barstow, 660
- Marrus, Michael R. (R), 691
- Marsh, Margaret S., "Anarchist Women, 1870–1920," 1330
- Marsh, Peter T. (R), 398, 981
- Marshall, George C., 776
- Marshall, P. J., and Glyndwr Williams, "The Great Map of Mankind: Perceptions of New Worlds in the Age of Enlightenment," 971
- "Marshall: Hero for Our Times," by Mosley, 776
- Martel, Leon, "Lend-Lease, Loans, and the Coming of the Cold War: A Study of the Implementation of Foreign Policy," 211
- Martin, Albro (C), 800
- Martin, Bernd, editor, "Die deutsche Beraterschaft in China, 1927–1938: Militär—Wirtschaft—Aussenpolitik" (E), 234
- Martin, Rex (R), 73
- Martindale, J. R., "The Prosopography of the Later Roman Empire," vol. 2: "A.D. 395–527," 659
- "Martin's Hundred," by Hume, 170
- Marty, Martin E. (R), 744
- Marx, Karl, 77
- "Marx: The First Hundred Years," edited by McLellan (E), 1356
- "Marxism and Modernism," by Lunn, 1245
- "Marxism and the Reality of Power, 1919–1980," by Narkiewicz, 363
- "Marxist Literary Thought in China," by Pickowicz, 155
- Masaryk, Thomas G., 713
- Masek, Rosemary (R), 387
- Mason, T. W. (C), 1143
- Mass, Jeffrey P., editor, "Court and Bakufu in Japan: Essays in Kamakura History" (E), 1124
- "Materiály k politickým, hospodářským a sociálním dějinám Československa v letech 1918–1929," by Harna, 712
- Mattheisen, Donald J. (R), 1008
- Mattheisen, Donald J., *History as Current Events: Recent Works on the German Revolution of 1848* (Review Essay), 1219–37
- Maucl, H. E., "Slavers in Paradise: Peruvian Slave Trade in Polynesia, 1862–1864," 510
- Maurus, Hrabanus, 969
- "Maximilian I (1459–1519)," by Benecke, 1289
- Maximilian I, Herzog and Kurfürst, 1007
- May, Dean L., "From New Deal to New Economics: The American Liberal Response to the Recession of 1937," 495
- May, Elaine Tyler (R), 485, 1080
- May, Henry F. (R), 195, 1330
- May, Henry F., editor, "Ideas, Faiths, and Feelings: Essays on American Intellectual and Religious History, 1952–1982" (E), 1358
- May, Irvin M., Jr., and Henry C. Dethloff, editors, "Southwestern Agriculture: Pre-Columbian to Modern," 758
- May, Robert E. (R), 1067
- Mavr, Otto, and Robert C. Post, editors, "Yankee Enterprise: The Rise of the American System of Manufactures," 468
- M'Bokolo, Elikia, "Noirs et Blancs en Afrique en Afrique Équatoriale: Les sociétés côtières et la pénétration française (vers 1820–1874)," 446
- Meaney, Neville (R), 498
- Mears, John A. (R), 424
- "Media and the American Mind," by Czitrom, 745
- "Medical Thinking," by King, 654
- MEDICINE AND MEDICAL HISTORY: Berridge and Edwards, "Opium and the People: Opiate Use in Nineteenth-Century England," 396; Bliss, "The Discovery of Insulin," 964; Courtwright, "Dark Paradise: Opiate Addiction in America before 1940," 169; Eccles, "Obstetrics and Gynecology in Tudor and Stuart England," 387; Fellman, "Making Sense of Self: Medical Advice Literature in Late Nineteenth-Century America," 194; King, "Medical Thinking," 654;
- "Medieval and Renaissance Studies," edited by Tirro (E), 233
- "Medieval English Clothmaking," by Bridbury, 1256
- "Medieval Kingship," by Myers, 91
- "Medieval Slavery and Liberation," by Dockès, 372
- Meehan-Waters, Brenda, "Autocracy and Aristocracy: The Russian Service Elite of 1730," 437
- Meeks, Wayne A., "The First Urban Christians: The Social World of the Apostle Paul," 1253
- Meier, Peter Uffe, "Omkring de fire species: Dansk merkantilistisk stabel- og navigationspolitik i 1720'erne," 1005

- Meiland, Jack W., and Michael Krausz, editors, "Relativism: Cognitive and Moral" (E), 231
- Mejcher, Helmut, and Alexander Schölch, editors, "Die Palästina-Frage, 1917–1948: Historische Ursprünge und internationale Dimensionen eines Nationenkonflikts" (E), 235
- Melton, James Van Horn (R), 1285
- "Men against Time," by Wood, 1245
- Mendelsohn, Ezra, "Zionism in Poland: The Formative Years, 1915–1926," 143
- Menefee, Samuel Pyeatt, "Wives for Sale: An Ethnographic Study of British Popular Divorce," 109
- Menk, Gerhard, "Die Hohe Schule Herborn in ihrer Frühzeit, 1584–1660: Ein Beitrag zum Hochschulwesen des deutschen Calvinismus im Zeitalter der Gegenreformation," 1007
- "Mennonites in Canada, 1920–1940," by Epp, 1110
- "Menominee Drums: Tribal Termination and Restoration," by Peroff, 501
- "Mercantilism as a Rent-Seeking Society," by Ekelund and Tollison, 81
- "The Merkabah in Rabbinic Literature," by Halperin, 371
- Merkel, Peter H. (R), 1016
- Merriman, John M., editor, "French Cities in the Nineteenth Century" (E), 788
- Merriman, John X., 730
- Meskill, John, "Academies in Ming China: A Historical Essay," 450
- "Mesmerism and the American Cure of Souls," by Fuller, 1065
- Messer, Robert L., "The End of an Alliance: James F. Byrnes, Roosevelt, Truman, and the Origins of the Cold War," by Messer, 212
- Metcalf, Barbara Daly, "Islamic Revival in British India: Deoband, 1860–1900," 1050
- "Metropolitan London: Politics and Urban Change, 1837–1981," by Garside and Young, 1267
- "The Mexican Frontier, 1821–1846: The American Southwest under Mexico," by Weber, 504
- "Mexico: From Independence to Revolution," edited by Raat (E), 235
- "Mexiko im 20. Jahrhundert," by Mols, 508
- Michael, Franz, "Rule by Incarnation: Tibetan Buddhism and Its Role in Society and State," 453
- Michel, Marc, "L'appel à l'Afrique: Contributions et réactions à l'effort de guerre en A.O.F., 1914–1919," 728
- "Michel Foucault," by Dreyfus and Rabinow, 648
- "Mid-America's Promise: A Profile of Kansas City Jewry," edited by Schultz, 769
- Middlekauff, Robert, "The Glorious Cause: The American Revolution, 1763–1789," 1060
- Middleton, Charles R. (R), 677
- Midelfort, H. C. Erik (R), 692
- Miers, Suzanne (R), 222
- "Migration and Ethnicity in Urban India," by Lewandowski, 456
- "Mikhail Bakunin: A Study in the Psychology and Politics of Utopianism," by Kelly, 1243
- Milenkovich, Michael M. (R), 77, 363
- Milikan, Robert, 209
- Miller, Arnold, 1096
- Miller, Arthur Selwyn, "Toward Increased Judicial Activism: The Political Role of the Supreme Court," 1107
- Miller, Char, "Fathers and Sons: The Bingham Family and the American Mission," 471
- Miller, D. A. (R), 81
- Miller, Darlis A., "The California Column in New Mexico," 1068
- Miller, Donald L. (R), 471
- Miller, Forrest A. (R), 1027
- Miller, John E., "Governor Philip F. La Follette, the Wisconsin Progressives, and the New Deal," 1097
- Miller, Sally M. (R), 491
- Miller, Sally M., editor, "Flawed Liberation: Socialism and Feminism" (E), 1356
- Miller, Stuart Creighton, "'Benevolent Assimilation': The American Conquest of the Philippines, 1899–1903," 1334
- Millet, Hélène, "Les chanoines du chapitre cathédral de Laon, 1272–1412," 1257
- Mills, Dennis (R), 400
- Milner, A. C., "Karajaan: Malay Political Culture on the Eve of Colonial Rule," 458
- Milner, Clyde A., II, "With Good Intentions: Quaker Work among the Pawnees, Otos, and Omahas in the 1870s," 1078
- MILITARY AND NAVAL HISTORY: Alpert, "La reforma militar de Azaña, 1931–1933," 1002; Armstrong, "Bullets and Bureaucrats: The Machine Gun and the United States Army, 1861–1916," 1094; Bachrach, *The Angevin Strategy of Castle Building in the Reign of Fulk Nerra*, 533–60; Barraclough, "From Agadir to Armageddon: Anatomy of a Crisis," 1247; Boockmann, "Der deutsche Orden," 96; Cavallie, "De höga officerarna," 695; Childs, "Armies and Warfare in Europe, 1648–1789," 1242; Cress, "Citizens in Arms: The Army and the Militia in American Society to the War of 1812," 751; Erlich, "The Struggle over Eritrea, 1962–1978," 1305; Fingard, "Jack in Port: Sailortowns of Eastern Canada," 1111; Frankenstein, "Le prix du réarmement français, 1935–1939," 999; Gillingham, "The Wars of the Roses," 661; Harlow, "California Conquered," 1068; Harms, *The Wars of August: Diagonal Narratives in African History*, 809–34; Hassler, "With Shield and Sword: American Military Affairs, Colonial Times to the Present," 1315; Jencks, "From Muskets to Missiles: Politics and Professionalism in the Chinese Army, 1945–1981," 156; Krausnick and Wilhelm, "Die Truppe des Weltanschauungskrieges," 422; Lael, "The Yamashita Precedent: War Crimes and Command Responsibility," 1249; McWhiney and Jamieson, "Attack and Die: Civil War Military Tactics and the Southern Heritage," 475; Miller, "The California Column in New Mexico," 1068; Morrow, "German Air Power in World War I," 1014; Mosley, "Marshall: Hero for Our Times," 776; Najita and Koschmann, editors, "Conflict in Modern Japanese History," 1047; Ose, "Entscheidung im Westen, 1944," 707; Porch, "The Conquest of Morocco," 1304; Porch, "The March to the Marne: The French Army, 1871–1914," 407; Prange, "At Dawn We Slept: The Untold Story of Pearl Harbor," 496; Richardson, "Comintern Army: The International Brigades and the Spanish Civil War," 693; Rothenberg, "Napoleon's Great Adversary: The Archduke Charles and the Austrian Army, 1792–1814," 426; Seward, "The Hundred Years War," 968; Spencer, "The Confederate Navy in Europe," 1325; Starr, "The Union Cavalry in the Civil War," vol. 2: "The War in the East from Gettysburg to Appomattox," 1069; Sundhaussen, "The Road to Power: Indonesian Military Politics, 1945–1967," 740; Toland, "Infamy: Pearl Harbor and Its Aftermath," 496; Tombs, "The War against Paris, 1871," 127; Van Creveld, "Fighting Power: German and U.S. Army Performance, 1939–1945," 1287; Wegner, "Hitlers Politische Soldaten," 1287
- "The Mimetic Tradition of Reform in the West," by

- Morrison, 646
 Minault, Gail, "The Khilafat Movement: Religious Symbolism and Political Mobilization in India," 737
 Minear, Richard H. (R), 733
 Miner, H. Craig, "Wichita: The Early Years, 1865–80," 1327
 "The Miners' Fight for Democracy," by Clark, 1096
 "Miners, Millhands, and Mountaineers: Industrialization of the Appalachian South," by Eller, 771
 "Ministers of Reform: The Progressives' Achievement," by Crunden, 762
 Mints, I. I., and A. P. Nenarokov, editors, "Zhenshchiny—Revolutsionery i uchenye," 721
 Mintz, Jerome R., "The Anarchists of Casa Vieja," 1276
 "Miracles and the Medieval Mind," by Ward, 967
 Miroff, Bruce (R), 1347
 "Mirror of the Indies," by Nieuwenhuys, 1052
 "Mirrors of Mortality: Studies in the Social History of Death," edited by Whaley (E), 514
 Mitchell, Leslie (R), 979
 Mitchell, Richard H. (R), 1047
 "MITI and the Japanese Miracle," by Johnson, 158
 Mitrakos, Alexander S., "France in Greece during World War I: A Study in the Politics of Power," 711
 Mitre, Bartolomé, 1119
 Miño Grijalva, Manuel, *et al.*, "Tres aspectos de la presencia española en México durante el porfiriato: Relaciones económicas, comerciantes y población," 226
 Mock, Wolfgang, and Wolfgang J. Mommsen, editors, "Die Entstehung des Wohlfahrtsstaates in Grossbritannien und Deutschland, 1850–1950," 976
 Mock, Wolfgang, "Imperiale Herrschaft und nationales Interesse: 'Constructive Imperialism' oder Freihandel in Grossbritannien vor dem Ersten Weltkrieg," 1269
 "A Moderate among Extremists: Dwight D. Eisenhower and the School Desegregation Crisis," by Duram, 1345
 "Moderate Puritans and the Elizabethan Church," by Lake, 670
 "Modern Authoritarianism," by Perlmutter, 652
 "Modern European Intellectual History," edited by LaCapra and Kaplan (E), 787
 "Modern Islamic Political Thought," by Enayat, 724
 "The Modern World-System," vol. 2: "Mercantilism," by Wallerstein, 81
 MODERNIZATION: Harding, "Organizing China: The Problem of Bureaucracy, 1949–1976," 453; Janos, "The Politics of Backwardness in Hungary, 1825–1945," 433; Lunn, "Marxism and Modernism," 1245; Weiss, "The Making of Technological Man," 127
 Moehring, Eugene P., "Public Works and the Patterns of Urban Real Estate Growth in Manhattan, 1835–1894," 489
 Moir, Robert E. (C), 1140
 Molho, Anthony (R), 1291
 Mols, Manfred, "Mexiko im 20. Jahrhundert: Politisches System, Regierungsprozess, und politische Partizipation," 508
 Molyneux, John, "Leon Trotsky's Theory of Revolution," 79
 Mommsen, Wolfgang J., and Gerhard Hirschfeld, editors, "Social Protest, Violence, and Terror in Nineteenth- and Twentieth-Century Europe" (E), 1356
 Mommsen, Wolfgang J., and Wolfgang Mock, editors, "Die Entstehung des Wohlfahrtsstaates in Grossbritannien und Deutschland, 1850–1950," 976
 Monash, John, 1054
 MONASTICISM: Rosenwein, "Rhinoceros Bound: Cluny in the Tenth Century," 99; Ultee, "The Abbey of St. Germain des Prés in the Seventeenth Century," 123;
 "The Monetary Policy of Fourteenth-Century Florence," by Cipolla, 1291
 "Monety Moskovskoi Rusi," by Fedorov-Davydov, 144
 Monkkonen, Eric H. (R), 488
 Monkkonen, Eric H., "Police in Urban America, 1860–1920," 190
 Monteón, Michael, "Chile in the Nitrate Era: The Evolution of Economic Dependence, 1880–1930," 1351
 Montezuma, Carlos, 1078
 Montias, John Michael, "Artists and Artisans in Delft: A Socio-Economic Study of the Seventeenth Century," 411
 Monticone, Alberto, "Deutschland und die Neutralität Italiens, 1914–1915," 1014
 Moody, T. W., "Davitt and Irish Revolution, 1846–82," 401
 Moogk, Peter N. (R), 219, 1348
 Moore, Deborah Dash, "B'nai B'rith and the Challenge of Ethnic Leadership," 197
 Moore, John C. (C), 245
 Moore, Winfred B., Jr., and Walter J. Fraser, Jr., editors, "From the Old South to the New: Essays on the Transitional South," 187
 "Moral Idealists, Bureaucracy, and Catherine the Great," by Gleason, 1025
 Moran, Emilio F., editor, "The Dilemma of Amazonian Development" (E), 1358
 Morgan, H. Wayne (R), 775
 Morgan, H. Wayne, "Drugs in America: A Social History, 1800–1980," 169
 Morgan, Kenneth O., "Consensus and Disunity: The Lloyd George Coalition Government, 1918–1922," 118
 Morgan, William James (R), 1325
 Moritz, Werner, "Die bürgerlichen Fürsorgeanstalten der Reichsstadt Frankfurt a.M. im späten Mittelalter," 377
 Morn, Frank, "'The Eye That Never Sleeps': A History of the Pinkerton National Detective Agency," 488
 Morris, Lewis, 174
 Morris, Richard B. (R), 466
 Morrison, Karl F., "The Mimetic Tradition of Reform in the West," 646
 Morrow, John H., Jr., "German Air Power in World War I," 1014
 Morse, Richard M. (R), 511
 Morse, Samuel, 745
 Morton, Desmond, "A Peculiar Kind of Politics: Canada's Overseas Ministry in the First World War," 1112
 Morton, Harry, "The Whale's Wake," 1312
 Morton, James Douglas, Earl of, 683
 Moseley, James G. (R), 193
 Mosley, Leonard, "Marshall: Hero for Our Times," 776
 "Il movimento popolare nell'ultimo secolo della Repubblica," by Perelli, 1254
 Mozzarelli, Cesare, "Sovrano, società e amministrazione locale nella Lombardia teresiana, 1749–1758," 1293
 Muccigrosso, Robert (R), 195
 Muldoon, James (R), 83
 Muldowny, John, and Michael J. McDonald, "TVA and the Dispossessed: The Resettlement of

- Population in the Norris Dam Area," 1341
 Mulholland, James A., "A History of Metals in Colonial America," 1318
 Muller, H. Nicholas III, and John J. Duffy, "An Anxious Democracy: Aspects of the 1830s," 1063
 Mundy, John Hine (R), 663
 Munholland, Kim (R), 1274
 "Municipal Reform and the Industrial City," edited by Fraser, 680
 Munro, John H. (R), 661
 Munsterberg, Hugo (R), 735
 "The Murdered Magicians: The Templars and Their Myth," by Partner, 970
 Murdock, Eugene E., "Ban Johnson: Czar of Baseball," 1095
 Murphy, Bruce Allen, "The Brandeis-Frankfurter Connection: The Secret Political Activities of Two Supreme Court Justices," 204
 Murphy, Orville T., "Charles Gravier, Comte de Vergennes: French Diplomacy in the Age of Revolution, 1719-1787," 996
 Murphy, Paul L. (R), 1107
 Murrah, David J., "C. C. Slaughter: Rancher, Banker, Baptist," 1074
 Murray, David R. (R), 503
 Murray, David R., "Odious Commerce: Britain, Spain, and the Abolition of the Cuban Slave Trade," 222
 "The Muses Flee Hitler: Cultural Transfer and Adaptation, 1930-1945," edited by Jackman and Borden (E), 1122
 Music: Vail and White, *Forms of Resistance: Songs and Perceptions of Power in Colonial Mozambique*, 883-919
 "The Muslim Discovery of Europe," by Lewis, 439
 "Mussolini Unleashed, 1939-1941," by Knox, 138
 "Mussolini," by Smith, 429
 Muste, A. J., 206
 Musto, David F. (R), 396
 Myers, Constance Ashton (R), 1330
 Myers, Henry A., "Medieval Kingship," 91
 Myres, Sandra L., "Westerling Women and the Frontier Experience, 1800-1915," 1079
 "Mysticism and Dissent," by Bayat, 723
- Nagel, Paul C. (R), 196, 1077
 Nagel, Paul C., "Descent from Glory: Four Generations of the John Adams Family," 1075
 Nagle, D. Brendan (R), 657
 Najemy, John M., "Corporatism and Consensus in Florentine Electoral Politics, 1280-1400," 1259
 Najita, Tetsuo, and J. Victor Koschmann, editors, "Conflict in Modern Japanese History: The Neglected Tradition," 1047
 Nakamura, Takufusa, "The Postwar Japanese Economy: Its Development and Structure," 1308
 "The Name of Brooke: The End of White Rajah Rule in Sarawak," by Reece, 1053
 "The Nan-chao Kingdom and T'ang and China's Southwestern Frontier," by Backus, 457
 "Napoleon's Great Adversary: The Archduke Charles and the Austrian Army, 1792-1814," by Rothenberg, 426
 Naqvi, Hamida Khatoon (R), 735
 Narkiewicz, Olga A. (R), 76
 Narkiewicz, Olga A., "Marxism and the Reality of Power, 1919-1980," 363
 Nash, Gerald D. (R), 492
 Nash, Roderick (R), 759
 "The National Charity Company," by Bahmueller, 979
 "The National Union of Women's Suffrage Societies, 1897-1914," by Hume, 986
 NATIONALISM: Armstrong, "Nations before Nationalism," 650; Boyce, "Nationalism in Ireland," 121; Breuilly, "Nationalism and the State," 961; De Bruin, "Islam en nationalisme in door Japan bezet Indonesië, 1942-1945," 1312; Gras and Gras, "La révolte des régions d'Europe de 1916 à nos jours," 385; Hardiman, "Peasant Nationalists of Gujarat," 456; Rousset, "Communisme et nationalisme Vietnamien," 1054; Voss, "On the Periphery of Nineteenth-Century Mexico: Sonora and Sinaloa, 1810-1877," 505; Yeager, "Barros Arana's *Historia jeneral de Chile*," 229
 "Nationalism and the State," by Breuilly, 961
 "Nationalism in Ireland," by Boyce, 121
 "Nationbuilding and the Politics of Nationalism: Essays on Austrian Galicia," edited by Markovits and Sysyn (E), 1122
 "Nations before Nationalism," by Armstrong, 650
Natives and Immigrants, Free Men and Slaves: Urban Workingmen in the Antebellum South, by Berlin and Gutman, 1175-1200
 "NATO after Thirty Years," edited by Kaplan and Clawson (E), 232
 Natoli, Claudio, "La Terza Internazionale e il fascismo, 1919-1923: Proletario di fabbrica e reazione industriale nel primo dopoguerra," 384
 Navarro, Marysa, and Nicholas Fraser, "Eva Perón," 513
 Neal, Claude, 772
 Neale, R. S. (R), 982
 Neely, Mark E., Jr., "The Abraham Lincoln Encyclopedia," 754
 Neil, J. Meredith (R), 1100
 Nelson, Daniel (R), 1102
 Nelson, William E., "The Roots of American Bureaucracy, 1830-1900," 1063
 Nenarokov, A. P., and I. I. Mints, editors, "Zhenshchiny—Revolutsionery i uchenye," 721
 Nesbit, Robert C. (R), 773
 "The Network of Control: State Supreme Courts and State Security Statutes, 1920-1970," by Jenson, 493
 Neuman, Mark, "The Speenhamland County: Poverty and the Poor Laws in Berkshire, 1782-1834," 1265
 Neusner, Jacob (R), 89
 "New Alliances, 1940-41," by Douglas, 364
 "The New Deal Lawyers," by Irons, 496
 "The New Empire of Diocletian and Constantine," by Barnes, 660
 "New Eye for the Navy: The Origin of Radar," by Allison, 210
 "New History: Studying Australia Today," edited by Osborne and Mandle (E), 516
 "New Opportunities in a New Nation: The Development of New York after the Revolution," edited by Jonas and Wells, 1061
 "New Perspectives on the Politics and Culture of Early Modern Scotland," edited by Dwyer *et al.* (E), 788
 Newbury, Colin (R), 510, 1312
 "News from the White House: The Presidential-Pres Relationship in the Progressive Era," by Juergens, 765
 "Newspapers and New Politics: Midwestern Municipal Reform, 1890-1900," by Nord, 1082
 Newton, Ronald C. (R), 230
 Nicholas, David (R), 1277
 "Nicolaus Cusanus," by Watts, 970
 Nicolet, Claude, "L'idée républicaine en France, 1789-1924: Essai d'histoire critique," 998
 Nieuwenhuys, Rob, "Mirror of the Indies: A History of Dutch Colonial Literature," 1052
 Nightingale, Florence, 397

- Nish, Ian, editor, "Anglo-Japanese Alienation, 1919–1952: Papers of the Anglo-Japanese Conference on the History of the Second World War" (E), 786
- Nishi, Toshio, "Unconditional Democracy: Education and Politics in Occupied Japan, 1945–1952," 159
- Nissenbaum, Stephen (R), 1316
- "The Nitrate Industry and Chile's Crucial Transition, 1870–1891," by O'Brien, 1351
- No Chosen People: The Myth of the Calvinist Origins of Afrikaner Nationalism and Racial Ideology*, by Du Toit, 920–52
- Noble, David W. (R), 762
- "Noblesse Oblige: Charity and Cultural Philanthropy in Chicago, 1849–1929," by McCarthy, 483
- Noel, Thomas J., "The City and the Saloon: Denver, 1858–1916," 757
- Noether, Emiliana P. (R), 1293
- "Noirs et Blancs en Afrique en Afrique Équatoriale," by M'Bokolo, 446
- Nolan, Mary, "Social Democracy and Society: Working-Class Radicalism in Düsseldorf, 1890–1920," 132
- Nolde, Emil, 419
- Noll, Mark A., and Nathan O. Hatch, editors, "The Bible in America: Essays in Cultural History," 744
- "The Nonconformist Conscience," by Bebbington, 111
- Noonan, Thomas S. (R), 144
- "The Nootka Connection: Europe and the Northwest Coast, 1790–1795," by Pethick, 778
- Nord, David Paul, "Newspapers and New Politics: Midwestern Municipal Reform, 1890–1900," 1082
- Norris, James D. (R), 1318
- North, Douglass C., "Structure and Change in Economic History," 360
- "Northampton," by Peck, 1262
- Northedge, F. S., and Audrey Wells, "British and Soviet Communism: The Impact of a Revolution," 682
- Norton, Mary Beth, "Liberty's Daughters: The Revolutionary Experience of American Women, 1750–1800," 750
- Nouailhat, Yves-Henri, "France et États-Unis, août 1914–avril 1917," 766
- "November 1918," by Brook-Shepherd, 382
- "Novgorodskaja feodal'naia votchina: Istorikogenealogicheskoe issledovanie," by Ianin, 717
- Nugent, Walter (R), 1329
- Numbers, Ronald L. (R), 194
- Numbers, Ronald L., editor, "Compulsory Health Insurance: The Continuing American Debate" (E), 791
- Nye, Robert A. (R), 105
- Nygård, Toivo, "Suomalainen äärioikeisto maailmansotien välillä: Ideologiset juuret, järjestöllinen perusta ja toimintamuodot," 1280
- Oakes, James, "The Ruling Race: A History of American Slaveholders," 1066
- Oates, Stephen B., "Let the Trumpet Sound: The Life of Martin Luther King, Jr.," 1108
- O'Brien, David J. (R), 208
- O'Brien, Joseph V., "Dear, Dirty Dublin: A City in Distress, 1899–1916," 685
- O'Brien, Michael, "McCarthy in Wisconsin," 217
- O'Brien, Michael, editor, "All Clever Men, Who Make Their Way: Critical Discourse in the Old South" (E), 791
- O'Brien, Patricia, "The Promise of Punishment: Prisons in Nineteenth-Century France," 689
- O'Brien, Thomas F., "The Nitrate Industry and Chile's Crucial Transition, 1870–1891," 1351
- "Obstetrics and Gynecology in Tudor and Stuart England," by Eccles, 387
- "Occupation Diplomacy: Britain, the United States, and Japan, 1945–1952," by Buckley, 1049
- "Ocherki istorii byta i kul'turnoi zhizni Rossii," by Semenova, 1024
- O'Connor, Feargus, 393
- "Odious Commerce," by Murray, 222
- O'Gorman, Frank, "The Emergence of the British Two-Party System, 1760–1832," 675
- O'Higgins, James, "Yves de Vallone: The Making of an *Esprit-Fort*," 699
- "Oil Booms: Social Change in Five Texas Towns," by Olien and Olien, 199
- Okenfuss, Max J. (R), 1025
- "Oklahoma Politics," by Scales and Goble, 1094
- "Old Age in Preindustrial Society," edited by Stearns (E), 1357
- "Old Dartmouth on Trial," by Tobias, 760
- Olien, Roger M., "From Token to Triumph: The Texas Republicans since 1920," 199
- Olien, Roger M., and Diana Davids Olien, "Oil Booms: Social Change in Five Texas Towns," 199
- "Olive Branch and Sword: The Compromise of 1833," by Peterson, 182
- Oliver, W. H., and B. R. Williams, editors, "The Oxford History of New Zealand," 163
- Olliff, Donathan C., "Reforma Mexico and the United States: A Search for Alternatives to Annexation, 1854–1861," 1067
- Olsen, Glenn W. (R), 660
- O'Malley, John W. (R), 1016
- "Omkring de fire species: Dansk merkantilistisk stabel- og navigationspolitik i 1720' rne," by Meier, 1005
- "On Britain," by Dahrendorf, 1261
- "On the Periphery of Nineteenth-Century Mexico: Sonora and Sinaloa, 1810–1877," by Voss, 505
- Once Again: Pearl Harbor, Microdots, and J. Edgar Hoover* (Research Note), by Bratzel and Rout, 953–60
- "A Once Charitable Enterprise: Hospitals and Health Care in Brooklyn and New York, 1885–1915," by Rosner, 763
- "One South: An Ethnic Approach to Regional Culture," by Reed (E), 516
- O'Neill, William L. (R), 1101
- O'Neill, William L., "A Better World: The Great Schism, Stalinism, and the American Intellectuals, 1337
- "Opium and the People: Opiate Use in Nineteenth-Century England," by Berridge and Edwards, 396
- Oppenheim, Janet (R), 977
- ORAL HISTORY: Curtin, *Laboratory for the Oral History of Slavery: The Island of Lamu on the Kenya Coast*, 858–82; Fadiman, "An Oral History of Tribal Warfare: The Meru of Mt. Kenya," 1039; Harms, *The Wars of August: Diagonal Narratives in African History*, 809–34; Humphries, "Hooligans or Rebels?" 116
- "An Oral History of Tribal Warfare: The Meru of Mt. Kenya," by Fadiman, 1039
- "The Organization of American Culture, 1700–1900," by Hall, 165
- "Organizing China: The Problem of Bureaucracy, 1949–1976," by Harding, 453
- Orgel, Stephen, and Guy Fitch Lytle, editors, "Patronage in the Renaissance" (E), 789
- "The Oriental Religions and American Thought," by Jackson, 193
- "Los orígenes del Partido Único en México," by

- Lajous, 1116
 "The Origins and Nature of the Scottish Enlightenment," edited by Campbell and Skinner (E), 516
 "The Origins of the American Business Corporation, 1784–1855," by Seavoy, 180
 "The Origins of American National Security Policy," by Brune, 1093
 "The Origins of the Korean War," by Cummings, 159
 "The Origins of the Peruvian Labor Movement, 1883–1919," by Blanchard, 1118
 Orkney, Robert Stewart, Earl of, 989
 Orlow, Dietrich (R), 422, 705
 Orr, Willie, "Deer Forests, Landlords, and Crofters: The Western Highlands in Victorian and Edwardian Times," 990
 Osborne, G., and W. F. Mandle, editors, "New History: Studying Australia Today" (E), 516
 Ose, Dieter, "Entscheidung im Westen, 1944: Der Oberbefehlshaber West und die Abwehr der alliierten Invasion," 707
 "Der österreichische Neoabsolutismus," by Brandt, 134
 "Our Common History: The Transformation of Europe," edited by Thompson (E), 1356
 Ourada, Patricia K. (R), 501
 "Ours Once More: Folklore, Ideology, and the Making of Modern Greece," by Herzfeld, 141
 "Outcast Cape Town," by Western, 731
 Outhwaite, R. B., editor, "Marriage and Society: Studies in the Social History of Marriage" (E), 514
 Overton, Richard C., "Perkins/Budd: Railway Statesmen of the Burlington," 1086
 Owen, David, "The Government of Victorian London, 1855–1889: The Metropolitan Board of Works, the Vestries, and the City Corporation," 1267
 "Oxford and the Ideal of Commonwealth," edited by Madden and Fieldhouse (E), 515
 "The Oxford History of New Zealand," edited by Oliver and Williams, 163

 Paasivirta, Juhani, "Finland and Europe: International Crises in the Period of Autonomy, 1808–1914," 414
 Pace, David (R), 420
 Pachter, Henry, "Weimar Etudes" (E), 1122
 Packard, Randall M. (R), 1305
 "Pacta ili Concordia od 1102. godine," by Antoljak, 103
 "Paesi e mari ignoti: America e colonialismo europeo nella cultura veneziana, secoli XVI–XVII," by Ambrosini, 1017
 Pagden, Anthony, "The Fall of Natural Man: The American Indian and the Origins of Comparative Ethnology," 1114
 Page, Stanley W., "The Geopolitics of Leninism," 76
 PALEOGRAPHY AND TEXTUAL EXEGESIS: Drews, "Basileus: The Evidence for Kingship in Geometric Greece," 1251; Golb and Pritsak, "Khazarian Hebrew Documents of the Tenth Century," 1260
 "Palestine and Israel in the Nineteenth and Twentieth Centuries," edited by Haim and Kedourie (E), 516
 Palmer, Bryan D., and Gregory S. Kealey, "Dreaming of What Might Be: The Knights of Labor in Ontario, 1880–1900," 1349
 "Palmerston: The Early Years, 1784–1841," by Bourne, 677
 "Die Palästina-Frage, 1917–1948," edited by Mejcher and Schölch (E), 235
 Pancake, John (R), 751
 Pankhurst, Richard (R), 1037
 "Papers concerning Robertson's Colony in Texas," vol. 9: "October 1834 through March 20, 1835," by McClean, 1322
 "The Papers of Woodrow Wilson," vols. 36–38, edited by Link *et al.*, 1091
 Park Chung Hee, 160
 "Parlamentarische Wahlen und Wahlsystem in der Weimarer Republik," by Schanbacher, 704
 "Parlamentarismus in Preussen, 1848/49–1857/58," by Grünthal, 1008
 "Parliament, the Press, and the Colonies, 1846–1880," by Sternbridge, 983
 Parmet, Herbert S. (R), 215
 Parrish, Michael E. (R), 203
 Parrish, Michael E., "Felix Frankfurter and His Times: The Reform Years," 205
 Parsons, Bill (R), 148
 Parsons, James B. (R), 450
 "Le parti catholique alsacien, 1890–1939," by Baechler, 1274
 Partner, Peter, "The Murdered Magicians: The Templars and Their Myth," 970
 "Past and Process in Herodotus and Thucydides," by Hunter, 87
 "Pat McCarran," by Edwards, 1099
 Patai, Raphael (R), 1239
 Patelos, Constantin G., "Vatican I et les évêques uniates: Un étape éclairante de la politique romaine à l'égard des orientaux, 1867–1870," 431
 "Patricians, Power, and Politics in Nineteenth-Century Towns," edited by Cannadine (E), 1121
 "Patronage in the Renaissance," edited by Lytle and Orgel (E), 789
 Patsavos, Christos C. (R), 1252
 Patterson, Henry, and Paul Bew, "Seán Lemass and the Making of Modern Ireland, 1945–66," 1272
 Patterson, Robert B. (R), 96
 Patterson, Stephen E. (R), 177
 "Pavia and Rome: The Lombard Monarchy and the Papacy in the Eighth Century," by Hallenbeck, 665
 Payne, Stanley G. (R), 409, 1002
 "Pazifismus in der Weimarer Republik," edited by Holl and Wette (E) 234
 "The Peace of Nicias and the Sicilian Expedition," by Kagan, 86
 Pearce, R. D., "The Turning Point in Africa: British Colonial Policy, 1938–48," 445
 Pearson, Lionel (R), 87
 "Peasant Nationalists of Gujarat," by Hardiman, 456
 "Peasant, State, and Society in Medieval South India," by Stein, 161
 "Peasants, Rebels, and Outcasts: The Underside of Modern Japan," by Hane, 157
 Peck, George T. (C), 247
 Peck, Linda Levy, "Northampton: Patronage and Policy at the Court of James I," 1262
 "A Peculiar Kind of Politics: Canada's Overseas Ministry in the First World War," by Morton, 1112
 Pegues, Franklin J. (R), 100
 "Péguy entre l'ordre et la révolution," by Leroy, 407
 "People and Power in Byzantium," by Kazhdan and Constable, 379
 "The People of the Colca Valley," by Cook, 783
 Perelli, Luciano, "Il movimento popolare nell'ultimo secolo della Repubblica," 1254
 "Period and Place: Research Methods in Historical Geography," edited by Baker and Billinge (E), 785
 "El Período parlamentario, 1861–1925," vol. 2: "Democracia y gobierno representativo," by González, 1118
 Perkin, Harold (R), 684

- Perkins, Charles E., 1086
 "Perkins/Budd: Railway Statesmen of the Burlington," by Overton, 1086
 Perlmutter, Amos, "Modern Authoritarianism: A Comparative Institutional Analysis," 652
 Peroff, Nicholas C. (R), 1107
 Peroff, Nicholas C., "Menominee Drums: Tribal Termination and Restoration," 501
 Peron, Eva, 513
 Perrolle, Pierre M. (R), 453
 "Personal Patronage under the Early Empire," by Saller, 657
 "Peru's Indian Peoples and the Challenge of Spanish Conquest: Huamanga to 1640," by Stern, 508
 Pessen, Edward (C), 805
 Petersen, William (R), 386
 Peterson, F. Ross (R), 1099
 Peterson, Merrill D., "Olive Branch and Sword: The Compromise of 1833," 182
 Pethick, Derek, "The Nootka Connection: Europe and the Northwest Coast, 1790–1795," 778
 Petry, Carl F. (R), 721
 Petry, Carl F., "The Civilian Elite of Cairo in the Later Middle Ages," 443
 Petter, Martin, and J. M. Lee, "The Colonial Office, War, and Development Policy: Organisation and the Planning of a Metropolitan Initiative, 1939–1945," 119
 Peyer, Hans Conrad, "Könige, Stadt und Kapital: Aufsätze zur Wirtschafts- und Sozialgeschichte des Mittelalters" (E), 1121
 "Philadelphia Communists, 1936–1956," by Lyons, 1101
 Philbin, Tobias R., "Admiral von Hipper: The Inconvenient Hero," 420
 "Philippine Social History," edited by McCoy and De Jesus, 1310
 Phillips, David (R), 115
 Phillips, Carla Rahn (R), 1000
 Phillips, Earl (R), 1036
 Phillips, J. R. S. (R), 968
 Phillips, John A., "Electoral Behavior in Unreformed England: Plumpers, Splitters, and Straights," 978
 Phillips, Roderick, "Divorce in New Zealand: A Social History," 164
 Phillips, William D., Jr. (R), 1276
 "Philo-Semitism and the Readmission of the Jews to England, 1603–1655," by Katz, 672
 "The *Philosophes* and Post-Revolutionary France," by Lough, 402
 PHILOSOPHY AND POLITICAL THEORY: Berman, "The Reenchantment of the World," 359; Browning, "Political and Constitutional Ideas of the Court Whigs" 673; Carlsnaes, "The Concept of Ideology and Political Analysis," 77; Dick, "Plurality of Worlds," 647; Dreyer, *Faith and Experience in the Thought of John Wesley*, 12–30; Dreyfus and Rabinow, "Michel Foucault," 648; Enayat, "Modern Islamic Political Thought," 724; Fletcher, *Cobden as Educator: The Free-Trade Internationalism of Eduard Bernstein, 1899–1914*, 561–78; Graham, "Between Science and Values," 359; Judd and Slinn, "The Evolution of the Modern Commonwealth, 1902–80," 988; Lough, "The *Philosophes* and Post-Revolutionary France, 402; Lubot, "Liberalism in an Illiberal Age: New Culture Liberals in Republican China, 1919–1937," 1045; Lustig, "Corporate Liberalism: The Origins of Modern American Political Theory, 1890–1920," 1083; McGiffert, *God's Controversy with Jacobean England*, 1151–74; Maravall, "Utopia y reformismo en la España de los Austrias," 1000; Nicolet, "L'idée républicaine en France, 1789–1924," 998; Schleich, "Aufklärung und Revolution," 402; Shkurinov, "Pozitivizm v Rossii XIX veka," 147; Szporluk, "The Political Thought of Thomas G. Masaryk," 713; Tillman, "Utilitarian Confucianism," 153; Young-Bruehl, "Hannah Arendt," 75
 Philp, Kenneth R. (R), 1078
 "The Phoenix of the Western World: Quetzalcoatl and the Sky Religion," by Brundage, 225
 PHYSICAL SCIENCES: Hufbauer, "The Formation of the German Chemical Community, 1720–1795," 417; Kargon, "The Rise of Robert Milikan," 209
 Pythian-Adams, Charles (R), 375
 Pickowicz, Paul G., "Marxist Literary Thought in China: The Influence of Ch'ü Ch'iu-pai," 155
 Pierson, Peter (R), 410
 Pike, David, "German Writers in Soviet Exile, 1933–1945," 133
 Pike, David Wingeate (R), 693
 Pike, Frederick B. (R), 512
 Pike, Frederick B., and Mark Falcoff, editors, "The Spanish Civil War, 1936–39: American Hemispheric Perspectives," 1353
 Pinkney, Alphonso (R), 197
 Pinoteau, Baron Hervé, "Vingt-cinq ans d'études dynastiques," 993
 Pintner, Walter M. (R), 1298
 Pipes, Daniel (R), 150
 "Plotting the Golden West: American Literature and the Rhetoric of the California Trail," by Fender, 183
 "Plurality of Worlds," by Dick, 647
 Podro, Michael, "The Critical Historians of Art," 1244
 Pois, Robert, "Emil Nolde," 419
 "Poland's Place in Europe: General Sikorski and the Origin of the Oder-Neisse Line, 1939–1943," by Terry, 1297
 "Polen—Das Ende der Erneuerung?" edited by Uschakow (E), 790
 "Police in Urban America, 1860–1920," by Monkkonen, 190
 "The Polite Escape: On the Myth of Secularization," by Ausmus, 358
 "Political and Constitutional Ideas of the Court Whigs," by Browning, 673
 "The Political and Social History of Khurasan under Abbasid Rule, 747–820," by Daniel, 149
 POLITICAL HISTORY: Abir, "Ethiopia and the Red Sea: The Rise and Decline of the Solomonic Dynasty," 727; Abrahamian, "Iran between Two Revolutions," 1303; Adanir, "Die Makedonische Frage," 1019; Andrews, "Presidential Government in Gaullist France," 692; Andrzejewski, "Socjaldemokratyczna partia wolnego miasta Gdańska, 1920–36," 437; Antoljak, "Pacta ili Concordia od 1102. godine," 103; Avrekh, "Tsarizm i IV Duma, 1912–1914 gg.," 1029; Bachrach, *The Angevin Strategy of Castle Building in the Reign of Fulk Nerra, 533–60*; Backus, "The Nan-chao Kingdom and T'ang and China's Southwestern Frontier," 457; Baldini, "Puntigli spagnoleschi e intrighi politici nella Roma di Clemente VIII," 709; Barnwell, "Love of Order: South Carolina's First Secession Crisis," 473; Batkay, "Authoritarian Politics in a Transitional State," 715; Bebbington, "The Nonconformist Conscience," 111; Bedeski, "State-Building in Modern China: The Kuomintang in the Prewar Period," 450; Bonney, "The King's Debts: Finance and Politics in France, 1589–1661," 123; Bottigheimer, "Ireland and the Irish," 990; Brandt, "Der österreichische Neoabsolutismus," 134; Brinkley, "Voices of Protest: Huey Long, Father

- Coughlin, and the Great Depression," 494; Bruneau, "The Church in Brazil: The Politics of Religion," 511; Burns, "The American Experiment: The Vineyard of Liberty," 460; Carcelius, "The Roots of Modern Egypt," 443; Chan, "The Glory and Fall of the Ming Dynasty," 448; Colley, "In Defiance of Oligarchy: The Tory Party, 1714–60," 390; Cookson, "The Friends of Peace: Anti-War Liberalism in England," 111; Craig, "The Germans," 1281; Cummings, "The Origins of the Korean War," 159; Daniel, "The Political and Social History of Khurasan under Abassid Rule, 747–820," 149; Daniell, "Colonial New Hampshire," 172; Davies, "God's Playground: A History of Poland," 436; Dawson, "Army Generals and Reconstruction," 186; De Madariaga, "Russia in the Age of Catherine the Great," 146; De Silva, "A History of Sri Lanka," 163; Donner, "The Early Islamic Conquests," 440; Dreyer, "Early Ming China," 449; Duffy and Muller, "An Anxious Democracy: Aspects of the 1830s," 1063; Dyson, "Red Harvest: The Communist Party and American Farmers," 208; Ekirch, "'Poor Carolina': Politics and Society in Colonial North Carolina, 1729–1776," 463; Garvin, "The Evolution of Irish Nationalist Politics," 121; Godfrey, "1204," 95; Golden, "The Godly Rebellion: Parisian Curés and the Religious Fronde, 1652–62," 402; Gomme *et al.*, "A Historical Commentary on Thucydides," vol. 5: "Book VIII," 367; González, "El Período parlamentario, 1861–1925," vol. 2: "Democracia y gobierno representativo," 1118; Goodway, "London Chartism, 1838–1848," 112; Greenhalgh, "Pompey: The Republican Prince," 656; Grossberg, "Japan's Renaissance: The Politics of the Muromachi Bakufu," 454; Hallenbeck, "Pavia and Rome: The Lombard Monarchy and the Papacy in the Eighth Century," 665; Hamilton, "The Limits of State Autonomy: Post-Revolutionary Mexico," 780; Hewitt, "Scotland under Morton, 1572–80," 683; Hilpert, "Kaiser- und Papstbriefe in den *Chronica majora* des Matthaeus Paris," 102; Housley, "The Italian Crusades," 378; Hovannisian, "The Republic of Armenia," vol. 2: "From Versailles to London, 1919–1920," 1032; Hutton, "The Cult of the Revolutionary Tradition," 126; Kagan, "The Peace of Nicias and the Sicilian Expedition," 86; Kamoche, "Imperial Trusteeship and Political Evolution in Kenya, 1923–1963," 151; Kazhdan and Constable, "People and Power in Byzantium," 379; Keddie, *The Iranian Revolution in Comparative Perspective*, 579–98; Kelly, "Victims, Authority, and Terror," 125; Kessler, "Politik, Kultur und Gesellschaft in Kroatien und Slawonien in der ersten Hälfte des 19. Jahrhunderts," 1020; Kilander, "Censur och propaganda: Svensk informationspolitik under 1900-talets första decennier," 412; Klein, "Bolivia: The Evolution of a Multi-Ethnic Society," 510; Kogan, "A Political History of Postwar Italy," 138; Krentz, "The Thirty at Athens," 654; Le Cordeur, "The Politics of Eastern Cape Separatism, 1820–1854," 152; Lewis, "Royal Succession in Capetian France," 99; Lilie, "Byzanz und die Kreuzfahrerstaaten," 95; Lippens, "A History of European Integration," vol. 1: "1945–1947," 365; Lorenz, "Eugen Richter," 418; McCormick, "The Presidential Game," 461; MacDonald, "The Making of an English Revolutionary: The Early Parliamentary Career of John Pym," 108; Macdougall, "James III," 662; Mendelsohn, "Zionism in Poland," 143; Milner, "Karajaan: Malay Political Culture on the Eve of Colonial Rule," 458; Mols, "Mexico im 20. Jahrhundert," 508; Morgan, "Consensus and Disunity: The Lloyd George Coalition Government," 118; Murphy, "The Brandeis-Frankfurter Connection," 204; Myers, "Medieval Kingship," 91; Najemy, "Corporatism and Consensus in Florentine Electoral Politics, 1280–1400," 1259; Narkiewicz, "Marxism and the Reality of Power, 1919–1980," 363; Nishi, "Unconditional Democracy: Education and Politics in Occupied Japan," 159; O'Brien, "McCarthy in Wisconsin," 217; Olien, "From Token to Triumph: The Texas Republicans since 1920," 199; Oliver and Williams, editors, "The Oxford History of New Zealand," 163; Parrish, "Felix Frankfurter and His Times," 205; Perelli, "Il movimento popolare nell'ultimo secolo della Repubblica," 1254; Peterson, "Olive Branch and Sword: The Compromise of 1833," 182; Pugh, "The Making of Modern British Politics, 1867–1939," 678; Reece, "The Name of Brooke: The End of White Rajah Rule in Sarawak," 1053; Reilly, "The Kingdom of León-Castilla under Queen Urraca, 1106–1126," 376; Richards, "A History of Highland Clearances," 400; Rosen, "Red Guard Factionalism and the Cultural Revolution in Guangzhou (Canton)," 156; Sargent, "Roosevelt and the Hundred Days," 494; Sartre, "Trois études sur l'Arabie romaine et byzantine," 370; Schilling, "Konfessionskonflikt und Staatsbildung," 415; Schoenbaum, "Zabern 1913," 131; Schwoerer, "The Declaration of Rights, 1689," 109; Shepelev, "Tsarizm i burzhuziia vo vtoroi polovine XIX veka," 438; Siewert, "Die Trittyen Attikas und die Heeresreform des Kleisthenes," 368; Speck, "Artabasdos, der rechtgläubige Vorkämpfer der göttlichen Lehren," 94; Speck, "The Butcher: The Duke of Cumberland and the Suppression of the 45," 684; Stein, "Peasant, State, and Society in Medieval South India," 161; Storing, editor, "The Complete Anti-Federalist," 467; Tariello, "The Reconstruction of American Political Ideology, 1865–1917," 492; Thompson and Prior, "South African Politics," 447; Valentić, "Vojna krajina i pitanje njezina sjedinjenja s Hrvatskom, 1849–1881," 432; Vital, "Zionism," 80; Wagner, "Brothers beyond the Sea: National Socialism in Canada," 220; Wallace, "British Government in Northern Ireland," 121; Watson, "Jacksonian Politics and Community Conflict," 181; Webster, "Rome against Caratacus," 89; Wiarda and Kryzanek, "The Dominican Republic," 503; Woolrych, "Commonwealth to Protectorate," 672; Wortman, "Government and Society in Central America," 780; Yang, "Korea and Two Regimes," 160
- "A Political History of Postwar Italy," by Kogan, 138
- POLITICAL SCIENCE: Andrews, "Presidential Government in Gaullist France," 692; Baechler, "Le parti catholique alsacien, 1890–1939," 1274; Clark, "The Dynamics of Change: The Crisis of the 1750s and English Party Systems," 674; Collier, "Regimes in Tropical Africa: Changing Forms of Supremacy, 1945–1975," 1034; Cox, "Lincoln and Black Freedom," 473; Dominick, "Wilhelm Liebknecht and the Founding of the German Social Democratic Party," 1012; Drummond, "The German Social Democrats in Opposition, 1949–1960," 1016; Fletcher, *Cobden as Educator: The Free-Trade Internationalism of Eduard Bernstein, 1899–1914*, 561–78; Hamilton, "Who Voted for Hitler?" 705; Heale, "The Presidential Quest," 468; Huard, "La préhistoire des partis: Le mouvement républicain

- en Bas-Languedoc, 1848–1881," 687; Lajous, "Los orígenes del Partido Único en México," 1116; Lerda, "Il Populismo americano," 1329; Maizlish and Kushma, editors, "Essays on American Antebellum Politics, 1840–1860," 1064; O'Gorman, "The Emergence of the British Two-Party System, 1760–1832," 675; Owen, "The Government of Victorian London, 1855–1889," 1267; Phillips, "Electoral Behavior in Unreformed England," 978; Schanbacher, "Parlamentarische Wahlen und Wahlsystem in der Weimarer Republik," 704; Vaughn, "The Anti-Masonic Party in the United States, 1826–1843," 1064; Young, "Ideology and Development in Africa," 445
- "The Political Thought of Thomas G. Masaryk," by Szporluk, 713
- "The Politician: The Life and Times of Lyndon Johnson, the Drive for Power," by Dugger, 502
- "The Politics of Backwardness in Hungary, 1825–1945," by Janos, 433
- "The Politics of Combined and Uneven Development," by Löwy, 76
- "The Politics of Eastern Cape Separatism, 1820–1854," by Le Cordeur, 152
- "The Politics of Ethnic Survival: Germans in Prague, 1861–1914," by Cohen, 434
- "The Politics of Indian Removal: Creek Government and Society in Crisis," by Green, 478
- "The Politics of Park Design," by Cranz, 1334
- "The Politics of Race in New York: The Struggle for Black Suffrage in the Civil War Era," by Field, 755
- "The Politics of Wilderness Preservation," by Allin, 759
- "Poliik, Kultur und Gesellschaft in Kroatien und Slawonien in der ersten Hälfte des 19. Jahrhunderts," by Kessler, 1020
- "Pompey: The Republican Prince," by Greenhalgh, 656
- "Pompey: The Roman Alexander," by Greenhalgh, 656
- Poni, Carlo, "Fossi e cavedagne benedicon le campagne," 708
- Pooley, Colin G., and James H. Johnson, editors, "The Structure of Nineteenth-Century Cities" (E), 787
- "'Poor Carolina': Politics and Society in Colonial North Carolina, 1729–1776," by Ekirch, 463
- "Popular Culture and Custom in Nineteenth-Century England," edited by Storch (E), 515
- "Popular Entertainment, Class, and Politics in Munich, 1900–1923," by Sackett, 1285
- "The Population History of England, 1541–1871," by Wrigley, 386
- "Il Populismo americano," by Lerda, 1329
- Porch, Douglas, "The Conquest of Morocco," 1304
- Porch, Douglas, "The March to the Marne: The French Army, 1871–1914," 407
- Porter, Andrew (R), 730
- "A Portrait Cast in Steel: Buckeye International and Columbus, Ohio," by Blackford, 1085
- Posnansky, Merrick (R), 726
- Posnansky, Merrick, and Christopher Ehret, editors, "The Archaeological and Linguistic Reconstruction of African History" (E), 1357
- Post, Robert C. (R), 209
- Post, Robert C., and Otto Mayr, editors, "Yankee Enterprise: The Rise of the American System of Manufactures," 468
- "Postscript to Victory," by Gajda, 120
- "The Postwar Japanese Economy," by Nakamura, 1308
- Poltholm, Christian P. (R), 445
- Potts, Louis W., "Arthur Lee: A Virtuous Revolutionary," 748
- Powell, James M. (R), 137, 378
- Powell, T. G. (R), 226
- "Power, Caste, and Law," by Rogoziński, 663
- Powicke, Michael R. (R), 968
- "Poziitivizm v Rossii XIX veka," by Shkurinov, 147
- Pozzetta, George E., and David R. Colburn, editors, "Reform and Reformers in the Progressive Era" (E), 1357
- "The Practice of Piety: Puritan Devotional Disciplines in Seventeenth-Century New England," by Hambrick-Stowe, 1317
- Prall, Stuart E. (R), 109
- Prange, Gordon W., "At Dawn We Slept: The Untold Story of Pearl Harbor," 496
- Prather, H. Leon, Sr., "Resurgent Politics and Educational Progressivism in the New South: North Carolina, 1890–1913," 1083
- "La préhistoire des partis: Le mouvement républicain en Bas-Languedoc, 1848–1881," by Huard, 687
- "Preservation Comes of Age: From Williamsburg to the National Trust," by Hosmer, 1100
- "The Presidential Game," by McCormick, 461
- "Presidential Government in Gaullist France," by Andrews, 692
- "The Presidential Quest," by Heale, 468
- "Press, Party, and Presidency," by Rubin, 746
- Pressly, Thomas J. (R), 184
- Pressly, William L., "The Life and Art of James Barry," 980
- Preston, Richard A. (R), 1112
- Preston, William, Jr. (R), 1104
- "Preussen: Eine Herausforderung," edited by Böhme (E), 234
- Preussen, Ronald W., "John Foster Dulles: The Road to Power," 217
- Price, J. L. (R), 411
- "A Priest in Public Service: Francis J. Haas and the New Deal," by Blantz, 1099
- "Priests and People in Pre-Famine Ireland, 1780–1845," by Connolly, 400
- "Prince Philipp Eulenburg-Hertefeld, 1847–1921," by Burmeister, 130
- Prior, Andrew, and Leonard Thompson, "South African Politics," 447
- Pritchard, Arnold (R), 671
- Pritchett, W. Kendrick, "The Greek State at War," vol. 3: "Religion," 88
- Pritsak, Omeljan, and Norman Golb, "Khazarian Hebrew Documents of the Tenth Century," 1260
- "Le prix du réarmement français, 1935–1939," by Frankenstein, 999
- "Problems of Cartesianism," edited by Lennon *et al.* (E), 785
- "Problemy srednevekovoi narodnoi kul'tury," by Gurevich, 666
- Prodi, Paolo, "Il sovrano pontefice—Un corpo e due anime: La monarchia papale nella prima età moderna," 1016
- "Prodigals and Pilgrims: The American Revolution against Patriarchal Authority," by Fliegelman, 465
- "The Promise of Punishment: Prisons in Nineteenth-Century France," by O'Brien, 689
- "La promotion des Juifs en France à l'époque du Second Empire, 1852–1870," by Cohen, 405
- Pronay, Nicholas, and D. W. Spring, editors, "Propaganda, Politics, and Film, 1918–45" (E), 515
- "Propaganda, Politics, and Film, 1918–45," edited by Pronay and Spring (E), 515
- PROSOPOGRAPHY: Bogue, "The Earnest Men: Republicans of the Civil War Senate," 185;

- Heydenreuter, "Der landesherrliche Hofrat unter Herzog und Kurfürst Maximilian I. von Bayern, 1598–1651," 1007; Honoré, "Emperors and Lawyers," 658; Martindale, "The Prosopography of the Later Roman Empire," vol. 2: "A.D. 395–527," 659
- "The Prosopography of the Later Roman Empire," vol. 2: "A.D. 395–527," by Martindale, 659
- "Prosperity and Misery in Modern Bengal," by Greenough, 1051
- "Protestant versus Catholic in Mid-Victorian England," by Arnstein, 113
- PROTESTANTISM: Beidelman, "Colonial Evangelism: A Socio-Historical Study of an East African Mission," 729; Bolton, "Southern Anglicanism: The Church of England in Colonial South Carolina," 1057; Epp, "Mennonites in Canada, 1920–1940," 1110; Hambrick-Stowe, "The Practice of Piety: Puritan Devotional Disciplines in Seventeenth-Century New England," 1317; Höpfl, "The Christian Polity of John Calvin," 698; Lake, "Moderate Puritans and the Elizabethan Church," 670; Thompson, "Tried as by Fire: Southern Baptists and the Religious Controversies of the 1920s," 1096;
- "O protestantismo, a maçonaria e a questão religiosa no Brasil," by Gueiros Vieira, 228
- Provenzo, Eugene F., Jr., and H. Warren Button, "History of Education and Culture in America," 1055
- Prowe, Diethelm (R), 1015
- Prucha, Francis Paul (R), 744
- "The Prussian Crusade," by Urban, 96
- "Il PSI, le riforme e la rivoluzione," by Vigezzi, 427
- "Pskovskaia Sudnaia gramota i ee vremia," by Alekseev, 145
- PSYCHOBIOGRAPHY AND PSYCHOHISTORY: Pois, "Emil Nolde," 419; Runyan, "Life Histories and Psychobiography," 1238; Stozier, "Lincoln's Quest for Union," 1070
- "Public Works and the Patterns of Urban Real Estate Growth in Manhattan, 1835–1894," by Moehring, 489
- Pugh, Martin, "The Making of Modern British Politics, 1867–1939," 678
- "Puntigli spagnoleschi e intrighi politici nella Roma di Clemente VIII," by Baldini, 709
- "Puritans and Predestination," by Wallace, 388
- "The Pursuit of Power: Technology, Armed Force, and Society since A.D. 1000," by McNeill, 1239
- Pym, John, 108
- Pyne, Stephen J., "Fire in America: A Cultural History of Wildland and Rural Fire," 759
- "Les Pyrénées et les Carpates," edited by Goy and Bobiriska (E), 233
- Quaife, Geoffrey R. (R), 164
- QUANTITATIVE HISTORY: Bogue, "The Earnest Men: Republicans of the Civil War Senate," 185; Clubb *et al.*, "Analyzing Electoral History," 168; Kleppner *et al.*, "The Evolution of American Electoral Systems," 168; Soltow and Stevens, "The Rise of Literacy and the Common School in the United States," 481; Weiss, "The Making of Technological Man," 127; Wrigley and Schofield, "The Population History of England, 1541–1871," 386
- "The Quebec and Acadian Diaspora in North America," edited by Breton and Savard (E), 791
- "La question des dettes interalliées et la reconstruction de l'Europe (1917–1929)," by Artaud, 106
- Quetzalcoatl, 225
- Quinn, David B., editor, "Early Maryland in a Wider World," 748
- Raat, W. Dirk, editor, "Mexico: From Independence to Revolution, 1810–1910" (E), 235
- Rabe, Stephen G., "The Road to OPEC: United States Relations with Venezuela, 1919–1976," 499
- Rabinow, Paul, and Hubert L. Dreyfus, "Michel Foucault: Beyond Structuralism and Hermeneutics," 648
- Rabinowitz, Howard N., editor, "Southern Black Leaders of the Reconstruction Era," 1071
- RACE AND RACE RELATIONS: Bacigalupo, "A Changing Perspective: Attitudes toward Creole Society in New Spain, 1521–1610," 1113; Balderrama, "In Defense of La Raza: The Los Angeles Mexican Consulate and the Mexican Community, 1929 to 1936," 774; Bawa, "Latin American Integration," 224; Buttlar, "Rassisch getrennte Schulen im Süden der USA, 1890–1950," 482; Cell, "The Highest Stage of White Supremacy," 965; Colburn and Jacoway, editors, "Southern Businessmen and Desegregation," 1343; Duram, "A Moderate among Extremists: Dwight D. Eisenhower and the School Desegregation Crisis," 1345; Janics, "Czechoslovak Policy and the Hungarian Minority, 1945–1948," 714; Lewsen, "John X. Merriman," 730; MacDougall, "Racial Myth in English History," 679; Western, "Outcast Cape Town," 731
- "Racial Myth in English History," by MacDougall, 679
- "The Radical Bourgeoisie: The Ligue de l'Enseignement," by Auspitz, 128
- "The Radical Left in Britain, 1939–1941," by Jupp, 399
- "The Radical Persuasion, 1890–1917: Aspects of the Intellectual History and the Historiography of Three American Radical Organizations," by Kraditor, 491
- RADICALISM: Auspitz, "The Radical Bourgeoisie: The Ligue de l'Enseignement," 128; Fishbein, "Rebels in Bohemia: The Radicals of *The Masses*, 1911–1917," 767; Hone, "For the Cause of Truth: Radicalism in London, 1796–1821," 390; Jupp, "The Radical Left in Britain, 1939–1941," 399; Kraditor, "The Radical Persuasion, 1890–1917," 491; Nolan, "Social Democracy and Society," 132; Royle and Walvin, "English Radicals and Reformers, 1760–1848," 979
- Rae, John B. (R), 86
- Ramirez, Bruno (R), 1090
- "Ramparts lointains: La politique française des travaux publics à Terre-Neuve et à l'Île Royale, 1695–1758," by Thorpe, 219
- "Ranchere Revolt: The Mexican Revolution in Guerrero," by Jacobs, 1351
- Ránki, György, and Iván T. Berend, "The European Periphery and Industrialization, 1780–1914," 975
- Rankin, Mary Backus (R), 733
- Ransel, David L. (R), 1024
- Ranum, Orest, and Robert Forster, editors, "Ritual, Religion, and the Sacred" (E), 232
- Rassekh, Nosratollah (R), 499
- "Rassisch getrennte Schulen im Süden der USA, 1890–1950," by Buttlar, 482
- Ravitch, Norman (R), 123
- Rawley, James A., "The Transatlantic Slave Trade: A History," 361
- Rawski, Evelyn S. (R), 1041
- Raychaudhuri, Tapan, and Irfan Habib, editors, "The Cambridge Economic History of India," vol. 1:

- "C. 1200–c. 1750," 735
 "The RCN in Retrospect, 1910–1968," edited by Boutilier (E), 791
 Rea, J. E. (R), 502
 Read, Colin, "The Rising in Western Upper Canada, 1837–8," 502
 Read, Donald, editor, "Edwardian England" (E), 1121
 "Rebellion and Riot: Popular Disorder in England during the Reign of Edward VI," by Beer, 670
 "Rebels and Rulers, 1500–1600," by Zagorin, 1241
 "Rebels in Bohemia: The Radicals of *The Masses*, 1911–1917," by Fishbein, 767
 "Reconstructing Europe after the Great War," by Silverman, 383
 "The Reconstruction of American Political Ideology, 1865–1917," by Tariello, 492
 "Red Guard Factionalism and the Cultural Revolution in Guangzhou (Canton)," by Rosen, 156
 "Red Harvest: The Communist Party and American Farmers," by Dyson, 208
 "Redemption by War," by Stromberg, 105
 Redford, Donald B. (R), 1250
 Reece, Jack E. (R), 385
 Reece, R. H. W., "The Name of Brooke: The End of White Rajah Rule in Sarawak," 1053
 Reed, John Shelton, "One South: An Ethnic Approach to Regional Culture" (E), 516
 "The Reenchantment of the World," by Berman, 359
 Reeves, A. C., "Lancastrian Englishmen," 98
 Reeves, Thomas C. (R), 217
 "Reform and Reformers in the Progressive Era," edited by Colburn and Pozzetta (E), 1357
 "Reforma Mexico and the United States," by Olliff, 1067
 "La reforma militar de Azaña, 1931–1933," by Alpert, 1002
 REFORMATION: Cowan, "The Scottish Reformation," 988; Loesch, "The Divine Community: Trinity, Church, and Ethics in Reformation Theologies," 380; Wright, "The Counter-Reformation," 698
 "Regimes in Tropical Africa: Changing Forms of Supremacy, 1945–1975," by Collier, 1034
 "Region, Race, and Reconstruction," edited by Kousser and McPherson, 476
 REGIONAL AND PROVINCIAL HISTORY: Berlin and Gutman, *Natives and Immigrants, Free Men and Slaves: Urban Workingmen in the Antebellum South*, 1175–1200; Buttlar, "Rassisch getrennte Schulen im Süden der USA, 1890–1950," 482; Cobb, "The Selling of the South: The Southern Crusade for Industrial Development," 1101; Demand, "Thebes in the Fifth Century," 1251; Edwards, "Christian Córdoba: The City and Its Region in the Late Middle Ages," 1276; Fraser and Moore, editors, "From the Old South to the New," 187; Goldfield, "Cotton Fields and Skyscrapers: Southern City and Region, 1607–1980," 1314; Hoffman and Albert, editors, "Sovereign States in an Age of Uncertainty," 1061; Jonas and Wells, editors, "New Opportunities in a New Nation: The Development of New York after the Revolution," 1061; Kousser and McPherson, editors, "Region, Race, and Reconstruction," 476; Limbaugh, "Rocky Mountain Carpetbaggers: Idaho's Territorial Governors," 757; Long, "The Saints and the Union: Utah Territory during the Civil War," 756; Madison, "The History of Indiana," vol. 5: "Indiana through Tradition and Change," 773; McDowell, "The Social Gospel in the South: The Woman's Home Mission Movement in the Methodist Episcopal Church," 1084; McWhiney and Jamieson, "Attack and Die: Civil War Military Tactics and the Southern Heritage," 475; Mann, "After the Gold Rush: Society in Grass Valley and Nevada City, California, 1849–1870," 1326; Prather, "Resurgent Politics and Educational Progressivism in the New South," 1083; Quinn, editor, "Early Maryland in a Wider World," 748; Read, "The Rising in Western Upper Canada, 1837–8," 502; Reitsma, "Centrifugal and Centripetal Forces in the Early Dutch Republic: The States of Overijssel, 1566–1600," 1278; Rosenberg, "The Code of the West," 477; Ruby and Brown, "Indians of the Pacific Northwest," 479; Scales and Goble, "Oklahoma Politics," 1094; Singal, "The War Within: From Victorian to Modernist Thought in the South," 772; Thompson, "Tried as by Fire: Southern Baptists and the Religious Controversies of the 1920s," 1096; Voss, "On the Periphery of Nineteenth-Century Mexico: Sonora and Sinaloa, 1810–1877," 505; Weber, "The Mexican Frontier, 1821–1826," 504; Wyatt-Brown, "Southern Honor: Ethics and Behavior in the Old South," 753
 Reichard, Gary W., and Robert H. Bremner, editors, "Reshaping America: Society and Institutions, 1945–1960," 1106
 Reid, Donald M., "Lawyers and Politics in the Arab World, 1880–1960," 150
 Reid, John Phillip (R), 747
 Reid, John Phillip, "In Defiance of the Law: The Standing-Army Controversy, the Two Constitutions, and the Coming of the American Revolution," 177
 Reilly, Bernard F., "The Kingdom of León-Castilla under Queen Urraca, 1109–1126," 376
 Reinerman, Alan J. (R), 709
 Reitsma, Richard, "Centrifugal and Centripetal Forces in the Early Dutch Republic: The States of Overijssel, 1566–1600," 1278
 "Relativism: Cognitive and Moral," edited by Meiland and Krausz (E), 231
 "Religion and Humanism," edited by Robbins (E), 785
 "Religion and Politics in Iran: Shi'ism from Quietism to Revolution," edited by Keddie (E), 1357
 "Religion and Resistance: East African Kingdoms in the Precolonial Period," by Berger, 150
 RELIGION AND THEOLOGY: Arnstein, "Protestant versus Catholic in Mid-Victorian England," 113; Bade, editor, "Imperialismus und Kolonialmission: Kaiserliches Deutschland und koloniale Imperium," 1011; Berger, "Religion and Resistance: East African Kingdoms in the Precolonial Period," 150; Besier, "Krieg—Frieden—Abrüstung: Die Haltung der europäischen und amerikanischen Kirchen zur Frage der deutschen Kriegsschuld, 1914–1933," 703; Brundage, "The Phoenix of the Western World: Quetzalcoatl and the Sky Religion," 225; Butler, "Gladstone—Church, State, and Tractarianism," 397; Bynum, "Jesus as Mother: Studies in the Spirituality of the High Middle Ages," 370; Cox, "The English Churches in a Secular Society," 983; Dreyer, *Faith and Experience in the Thought of John Wesley*, 12–30; Du Toit, *No Chosen People: The Myth of the Calvinist Origins of Afrikaner Nationalism and Racial Ideology*, 920–52; Estes, "Christian Magistrate and State Church: The Reforming Career of Johannes Brenz," 1006; Fishburn, "The Fatherhood of God and the Victorian Family," 192; Fuller, "Mesmerism and the American Cure of Souls," 1065; George, "John Robinson and the English Separatist Tradition," 1263; Gueiros Vieira, "O protestantismo, a maçonaria e a questão religiosa no Brasil," 228; Hobart, "Science and Religion in the

- Thought of Nicholas Malebranche," 75; Hornung, "Conceptions of God in Ancient Egypt," 1250; McCoy and De Jesus, editors, "Philippine Social History," 1310; McGiffert, *God's Controversy with Jacobean England*, 1151-74; Meeks, "The First Urban Christians: The Social World of the Apostle Paul," 1253; Minault, "The Khilafat Movement," 737; Pritchett, "The Greek State at War," vol. 3: "Religion," 88; Schilling, "Konfessionskonflikt und Staatsbildung," 415; Sullivan, "John Toland and the Deist Controversy," 389; Thomas, "Christianity in Roman Britain to A.D. 500," 967; Tucci, "The Religions of Tibet," 1309; Wallace, "Puritans and Predestination," 388; Ward, "Miracles and the Medieval Mind," 967
- "The Religions of Tibet," by Tucci, 1309
- RENAISSANCE: Brucker, *Tales of Two Cities: Florence and Venice in the Renaissance* (Review Essay), 599-616; Hobart, "Science and Religion in the Thought of Nicholas Malebranche," 75
- Renzi, William A., *Who Composed "Sazonov's Thirteen Points"? A Re-Examination of Russia's War Aims of 1914* (Research Note), 347-57
- "The Republic of Armenia," vol. 2: "From Versailles to London, 1919-1920," by Hovannisian, 1032
- "A Republic of Nobles: Studies in Polish History to 1864," edited by Fedorowicz *et al.* (E), 1123
- "Reshaping America: Society and Institutions, 1945-1960," edited by Bremner and Reichard, 1106
- "Resistance and Compromise," by Holmes, 671
- "Restauratione chiesa e società," by Semararo, 709
- "A Restless People: Americans in Rebellion, 1770-1787," by Handlin and Handlin, 178
- "Resurgent Politics and Educational Progressivism in the New South," by Prather, 1083
- Rétat, Pierre, editor, "Le journalisme d'Ancien Régime: Questions et propositions" (E), 233
- "Reverend Beecher and Mrs. Tilton: Sex and Class in Victorian America," by Waller, 484
- "La révolte des régions d'Europe occidentale de 1916 à nos jours," by Gras and Gras, 385
- REVOLUTIONS: Babikova, "Dvoevlastie v Sibiri," 719; Best, "War and Society in Revolutionary Europe, 1770-1870," 973; Christie, "Wars and Revolutions: Britain, 1760-1815," 675; Harrison, "The Endless War: Fifty Years of Struggle in Vietnam," 740; Hooglund, "Land and Revolution in Iran, 1960-1980," 1033; Huber, "The Revolutionary Origins of Modern Japan," 157; Hutton, "The Cult of the Revolutionary Tradition," 126; Jacobs, "Ranchero Revolt: The Mexican Revolution in Guerrero," 1351; Joseph, "Revolution from Without: Yucatán, Mexico, and the United States, 1880-1924," 505; Keddie, *The Iranian Revolution in Comparative Perspective*, 579-98; Kelley and Klein, "Revolution and the Rebirth of Inequality: A Theory Applied to the National Revolution in Bolivia," 227; Kennedy, "The Jacobin Clubs in the French Revolution," 404; Mattheisen, *History as Current Events: Recent Works on the German Revolution of 1848* (Review Essay), 1219-37; Middlekauff, "The Glorious Cause: The American Revolution," 1060; Mints and Nenarokov, editors, "Zhenshchiny—Revolutionary i uchenye," 721; Molyneux, "Leon Trotsky's Theory of Revolution," 79; Suny, *Toward a Social History of the October Revolution*, 31-52; Tuck, "The Holy War in Los Altos," 1116; Tucker and Hendrickson, "The Fall of the First British Empire: Origins of the War of American Independence," 1059; Zagorin, "Rebels and Rulers, 1500-1600," 1241
- "Revolution and the Rebirth of Inequality: A Theory Applied to the National Revolution in Bolivia," by Kelley and Klein, 227
- "Revolution from Without: Yucatán, Mexico, and the United States, 1880-1924," by Joseph, 505
- "The Revolutionary Origins of Modern Japan," by Huber, 157
- "Revolutions in Americans' Lives: A Demographic Perspective," by Wells, 743
- Reynolds, Edward (R), 1034
- "Rhinoceros Bound: Cluny in the Tenth Century," by Rosenwein, 99
- "Richard Morris Hunt," by Baker, 195
- "Richard Stanihurst," by Lennon, 991
- Richards, Eric, "A History of the Highland Clearances: Agrarian Transformation and the Evictions, 1746-1886," 400
- Richards, J. F., and Richard P. Tucker, editors, "Global Deforestation and the Nineteenth-Century World Economy" (E), 1120
- Richards, Jeffrey, "Consul of God: The Life and Times of Gregory the Great," 92
- Richardson, Elmo (R), 1087
- Richardson, Peter, "Chinese Mine Labour in the Transvaal," 1041
- Richardson, R. Dan, "Comintern Army: The International Brigades and the Spanish Civil War," 693
- Richardson, William (R), 508
- Richmond, Admiral Sir Herbert, 987
- Richter, Eugen, 418
- "The Right to Manage: Industrial Relations Policies of American Business in the 1940s," by Harris, 1102
- Righter, Robert W., "Crucible for Conservation: The Creation of Grand Teton National Park," 759
- Riley, G. Michael (R), 779
- Riley, P. W. J. (R), 684
- Rimmel, Lesley A. (C), 803
- Rimmer, Gordon (R), 104
- Rintala, Marvin (R), 1280
- Rischin, Moses (R), 1089
- "The Rise and Fall of the Political Press in Britain," by Koss, 113
- "The Rise of Colleges: Institutions of Learning in Islam in Islam and the West," by Makdisi, 442
- "The Rise of the International Organisation," by Armstrong, 1250
- "The Rise of Literacy and the Common School in the United States," by Soltow and Stevens, 481
- "The Rise of Robert Milikan," by Kargon, 209
- "The Rise of Suburbia," edited by Thompson, 680
- "The Rising in Western Upper Canada, 1837-8," by Read, 502
- "Ritual, Religion, and the Sacred," edited by Forster and Ranum (E), 232
- Rizvi, Gowher (R), 1050
- "The Road to OPEC: United States Relations with Venezuela, 1919-1976," by Rabe, 499
- "The Road to Power: Indonesian Military Politics, 1945-1967," by Sundhaussen, 740
- Roark, James L. (R), 1066
- Robbins, Keith, editor, "Religion and Humanism" (E), 785
- Robbins, William G., "Lumberjacks and Legislators: Political Economy of the U.S. Lumber Industry, 1890-1941," 1087
- "Robert Stewart, Earl of Orkney," by Anderson, 989
- Roberts, Brian R., and Peter J. Buckley, "European Direct Investment in the U.S.A. before World War I," 490
- Roberts, David D. (R), 384
- Robertson, Priscilla (R), 484
- Robertson, Priscilla, "An Experience of Women:

- Pattern and Change in Nineteenth-Century Europe," 104
- Robertson, Sterling C., 1322
- Robinson, Jo Ann Ooiman, "Abraham Went Out: A Biography of A. J. Muste," 206
- Robinson, John, 1263
- Robinson, John, "Index" to "The Complete Anti-Federalist," 467
- Robinson, John L., "Bartolomé Mitre: Historian of the Americans," 1119
- Robinson, Willard B. (R), 166
- Robinson-Hammerstein, Helga (R), 991
- Robson, L. L. (R), 741
- Rock, William, R. (R), 364
- "A Rock in a Weary Land: The African Methodist Episcopal Church during the Civil War and Reconstruction," by Walker, 476
- Rockefeller, Nelson, 1347
- "Rocky Mountain Carpetbaggers: Idaho's Territorial Governors," by Limbaugh, 757
- Rodnitzky, Jerry (R), 494
- Roebuck, Janet (R), 115
- Rogers, George C., Jr. (R), 463, 1323
- Rogin, Michael (R), 465
- Rogozinski, Jan, "Power, Caste, and Law: Social Conflict in Fourteenth-Century Montpellier," 663
- Rohe, Karl, editor, "Die Westmächte und das Dritte Reich, 1933-1939: Klassische Grossmachtrivalität oder Kampf zwischen Demokratie und Diktatur?" 1286
- Röhl, John C. G., and Nicolaus Sombart, editors, "Kaiser Wilhelm II: New Interpretations," 701
- Roider, Karl A., Jr., "Austria's Eastern Question, 1700-1790," 711
- Roland, Charles P. (R), 475
- Rolle, Andrew (R), 191
- Rollins, Alden (C), 530
- Rollins, Peter C., editor, "Hollywood as Historian: American Film in a Cultural Context" (E), 1358
- "Romania between East and West," edited by Fischer-Galati *et al.* (E), 789
- Romasco, Albert U. (R), 1097
- "Rome against Caratacus," by Webster, 89
- "Roosevelt and the Hundred Days: Struggle for the Early New Deal," by Sargent, 494
- "The Roots of American Bureaucracy, 1830-1900," by Nelson, 1063
- "The Roots of Modern Egypt," by Carcelius, 443
- "The Roots of Modern Japan," by Lehmann, 1047
- Rosand, David, and Robert W. Hanning, editors, "Castiglione: The Ideal and the Real in Renaissance Culture" (E), 1122
- Rose, Anne C. (R), 182
- Rose, Michael E. (R), 979
- Rosen, Andrew (R), 680
- Rosen, Ruth, "The Lost Sisterhood: Prostitution in America, 1900-1918," 1084
- Rosen, Stanley, "Red Guard Factionalism and the Cultural Revolution in Guangzhou (Canton)," 156
- Rosenberg, Bruce A., "The Code of the West," 477
- Rosenberg, Rosalind, "Beyond Separate Spheres: Intellectual Roots of Modern Feminism," 484
- Rosenberg, William G., and Marilyn B. Young, "Transforming Russia and China: Revolutionary Struggle in the Twentieth Century," 79
- Rosenstone, Robert A. (R), 767
- Rosenwein, Barbara H., "Rhinoceros Bound: Chiny in the Tenth Century," 99
- Rosner, David, "A Once Charitable Enterprise: Hospitals and Health Care in Brooklyn and New York, 1885-1915," 763
- "Rossiia i SShA: Ekonomicheskii, kul'turnye i diplomaticheskie sviazi, 1867-1881," by Kuropiatnik, 1300
- Rossiter, Margeret W., "Women Scientists in America: Struggles and Strategies to 1940," 1339
- Rotberg, Robert I. (R), 84
- Roth, Guenther (R), 132, 1012
- Rothenberg, Gunther E. (R), 1242
- Rothenberg, Gunther E., "Napoleon's Great Adversary: The Archduke Charles and the Austrian Army, 1792-1814," 426
- Rousset, Pierre, "Communisme et nationalisme Vietnamien: Le Vietnam entre les deux guerres mondiales," 1054
- Rout, Leslie, Jr., and John Bratzel, *Once Again: Pearl Harbor, Microdots, and J. Edgar Hoover* (Research Note), 953-60
- Rowen, Herbert H. (R), 1003
- Rowland, Daniel (R), 145
- "Royal Justice and the Medieval English Countryside," by Dewindt and Dewindt, 373
- "Royal Succession in Capetian France," by Lewis, 99
- Royle, Edward, and James Walvin, "English Radicals and Reformers, 1760-1848," 979
- Rovster, Charles (R), 748
- Rubin, Barry (R), 213
- Rubin, Richard L., "Press, Party, and Presidency," 746
- Ruby, Robert H., and John A. Brown, "Indians of the Pacific Northwest: A History," 479
- Rudolph, Frederick (R), 760
- Rudwick, Elliott (R), 1108
- Rudy, Willis (R), 218
- Ruestow, Edward G. (R), 75
- "Rule by Incarnation: Tibetan Buddhism and Its Role in Society and State," by Michael, 453
- "The Ruling Race: A History of American Slaveholders," by Oakes, 1066
- Runyan, William McKinley, "Life Histories and Psychobiography: Explorations in Theory and Method," 1238
- Russell, Conrad (C), 1140
- Russell-Wood, A. J. R., "The Black Man in Slavery and Freedom in Colonial Brazil," 1117
- Russett, Cynthia Eagle (R), 1328
- "Russia in the Age of Catherine the Great," by De Madariaga, 146
- "Russlands erste Nationalitäten," by Kappeler, 718
- "Russophobia in New Zealand, 1838-1908," by Barratt, 1313
- Ruud, Charles A., "Fighting Words: Imperial Censorship and the Russian Press, 1804-1906," 1298
- Ryan, Mary P., "Cradle of the Middle Class: The Family in Oneida County, New York, 1790-1865," 752
- Sack, James J. (R), 390
- Sackett, Robert Eben, "Popular Entertainment, Class, and Politics in Munich, 1900-1923," 1285
- Saija, Marcello, "Un 'soldino' contro il fascismo: Istituzioni ed élites politiche nella Sicilia del 1923," 428
- "Sailor-Scholar: Admiral Sir Herbert Richmond," by Hunt, 987
- "Saints and Society: The Two Worlds of Western Christendom, 1000-1700," by Weinstein and Bell, 1255
- "The Saints and the Union: Utah Territory during the Civil War," by Long, 756
- "Saints, Slaves, and Blacks: The Changing Place of Black People within Mormonism," by Bringham, 1255

- 189
 "Saladin," by Lyons and Jackson, 721
 Salisbury, Neal, "Manitou and Providence: Indians, Europeans, and the Making of New England, 1500–1643," 746
 Saller, Richard P., "Personal Patronage under the Early Empire," 657
 Salmon, E. T., "The Making of Roman Italy," 655
 Salmon, J. H. M. (R), 122
 Salvatore, Nick, "Eugene V. Debs: Citizen and Socialist," 1336
 Salway, Peter (R), 89
 "Samhällsförändring och sammanslutningsformer," by Jansson, 1279
 "Samoderzhavie i dvorianstvo v 1902–1907 gg," by Solov'ev, 1301
 Sandberg, Lars G. (R), 412
 Santos, James A., and Harry E. Cross, "Across the Border: Rural Development in Mexico and Recent Migration to the United States," 226
 Sandoz, Ellis, editor, "Eric Voegelin's Approach to Thought: A Critical Appraisal" (E), 514
 Sanford, Henry S., 1075
 Sargent, James E., "Roosevelt and the Hundred Days: Struggle for the Early New Deal," 494
 Sarna, Jonathan D. (C), 803
 Sarti, Roland (R), 1019
 Sartre, Maurice, "Trois études sur l'Arabie romaine et byzantine," 370
 Sater, William F. (R), 230
 Sather, Leland B. (R), 1005
 "Saudi-American Relations," by Grayson, 499
 Saunders, A. C. de C. M., "A Social History of Black Slaves and Freedmen in Portugal, 1441–1555," 410
 Saunders, Kay (R), 742
 Saunders, Kay, "Workers in Bondage: The Origins and Bases of Unfree Labour in Queensland, 1824–1916," 741
 Saunders, Paul, "Edward Jenner: The Cheltenham Years, 1795–1823; Being a Chronicle of the Vaccination Campaign," 676
 Sautter, Udo (R), 976
 Sauvage, Monique, and Jean-Noël Jeanneney, editors, "Télévision, nouvelle mémoire: Les magazines de grand reportage, 1959–1968," 1275
 Savage, William W., Jr. (R), 477
 Savage, William W., Jr., "The Cowboy Hero: His Image in American History and Culture," 756
 Savard, Pierre, and Raymond Breton, editors, "The Quebec and Acadian Diaspora in North America" (E), 791
 Savitt, Todd L. (R), 654
 Savory, Roger M. (R), 1302
 Scales, James R., and Danney Goble, "Oklahoma Politics: A History," 1094
 Scarpaci, J. Vincenza, and Robert F. Harney, editors, "Little Italies in North America," 191
 Scattergood, V. J., and J. W. Sherborne, editors, "English Court Culture in the Later Middle Ages" (E), 1121
 Schaffer, Daniel, "Garden Cities for America: The Radburn Experience," 770
 Schanbacher, Eberhard, "Parlamentarische Wahlen und Wahlsystem in der Weimarer Republik: Wahlgesetzgebung und Wahlreform im Reich und in den Ländern," 704
 Scharf, Lois, and Joan M. Jensen, editors, "Decades of Discontent: The Women's Movement, 1920–1940" (E), 1358
 Schek, Manfred, "Zwischen Weltkrieg und Revolution: Zur Geschichte der Arbeiterbewegung in Württemberg, 1914–1920," 132
 Scherer, Lester B. (R), 189
 Schiff, Warren (R), 1352
 Schilling, Heinz, "Konfessionskonflikt und Staatsbildung: Eine Fallstudie über das Verhältnis von religiösem und sozialem Wandel in der Frühneuzeit am Beispiel der Grafschaft Lippe," 415
 Schirach, Baldur von, 1288
 Schleich, Thomas, "Aufklärung und Revolution: Die Wirkungsgeschichte Gabriel Bonnot de Mablys in Frankreich, 1740–1914," 402
 Schlissel, Lillian (R), 1079
 Schlissel, Lillian, "Women's Diaries of the Westward Journey," 184
 Schmokel, Wolfe W. (R), 446
 Schneer, Jonathan (R), 399
 Schneer, Jonathan, and James E. Cronin, editors, "Social Conflict and the Political Order in Modern Britain" (E), 788
 Schneider, William H., "An Empire for the Masses: The French Popular Image of Africa, 1870–1900," 1274
 Schober, Richard, "Die Tiroler Frage auf der Friedenskonferenz von Saint Germain," 1290
 Schoenbaum, David, "Zabern 1913: Consensus Politics in Imperial Germany," 131
 Schofield, R. S., and E. A. Wrigley, "The Population History of England, 1541–1871: A Reconstruction," 386
 Schölch, Alexander, and Helmut Mejer, editors, "Die Palästina-Frage, 1917–1948: Historische Ursprünge und internationale Dimensionen eines Nationenkonflikts" (E), 235
 Scholz, Bernhard W. (R), 969
 Schönhoven, Klaus, and Dieter Langewiesche, editors, "Arbeiter in Deutschland: Studien zur Lebensweise der Arbeiterschaft im Zeitalter der Industrialisierung" (E), 234
 Schoonover, Thomas (R), 774
 Schoppa, R. Keith, "Chinese Elites and Political Change: Zhejiang Province in the Early Twentieth Century," 733
 Schreiber, Roy E. (R), 108
 Schuker, Stephen A. (R), 106, 383, 766
 Schulte Nordholt, Jan Willem, "The Dutch Republic and American Independence," 466
 Schultz, Joseph P., editor, "Mid-America's Promise: A Profile of Kansas City Jewry," 769
 Schulz, Gerhard, editor, "Geheimdienste und Widerstandsbewegungen im Zweiten Weltkrieg" (E), 786
 Schurz, Carl, 474
 Schutz, John A. (R), 1321
 Schwab, George, editor, "United States Foreign Policy at the Crossroads" (E), 1358
 Schwarz, Richard W. (R), 1066
 Schwarz, Robert (R), 1290
 Schwoerer, Lois G., "The Declaration of Rights, 1689," 109
 "Science and Religion in the Thought of Nicholas Malebranche," by Hobart, 75
 "Scienze, credenze occulte, livelli di cultura" (E), 786
 "Scotland under Morton, 1572–80," by Hewitt, 683
 Scott, H. M. (R), 973
 Scott, William B. (R), 770
 "The Scottish Reformation," by Cowan, 988
 Scribner, R. W. (R), 1005
 Sealey, Raphael (R), 1251
 "The Search for a New Order: Intellectuals and Fascism in Prewar Japan," by Fletcher, 734
 "Seán Lemass and the Making of Modern Ireland, 1945–66," by Bew and Patterson, 1272
 "Seasonable Revolutionary: The Mind of Charles

- Chauncy," by Lippy, 749
- Seaton, Douglas P., "Catholics and Radicals: The Association of Catholic Trade Unionists and the American Labor Movement, from Depression to Cold War," 208
- Seavoy, Ronald E., "The Origins of the American Business Corporation, 1784–1855: Broadening the Concept of Public Service during Industrialization," 180
- "The Secular Mind: Transformations of Faith in Modern Europe," edited by Wagar (E), 787
- "Sécularisation et religions politiques," by Sironneau, 358
- Sedgwick, Alexander (R), 1274
- "Seeds of Mideast Crisis: The United States Diplomatic Role in the Middle East during World War II," by Bryson, 213
- Sefton, David S. (R), 665
- Sehlinger, Peter J. (R), 1118
- Seigel, Jerrold (R), 1245
- Seip, Terry L. (R), 189
- Selement, George (R), 749
- "Self-Help in the 1890s Depression," by Grant, 1081
- Selleck, Roberta G. (R), 412
- "Selling Mothers' Milk: The Wet-Nursing Business in France, 1715–1914," by Sussman, 688
- "The Selling of the South: The Southern Crusade for Industrial Development," by Cobb, 1101
- Semamaro, Cosimo, "Restaurazione chiesa e società: La 'seconda ricupera' e la rinascita degli ordini religiosi nello Stato pontificio, Marche e Legazioni, 1815–1823," 709
- Semenova, L. N., "Ocherki istorii byta i kul'turnoi zhizni Rossii (Pervaia polovina XVIII v.)," 1024
- "Le Séminaire de Québec de 1760 à 1800," by Baillargeon, 1348
- Semmel, Bernard (R), 667
- Senior, Hereward (R), 990
- Senn, Alfred E. (C), 805
- SERFDOM AND THE PEASANTRY: Druzhimna, "Iuzhnaia Ukraina v period krizisa feodalizma, 1825–1860 gg.," 718; Glüer, "Christliche Theologie in China," 154; Spittler, "Verwaltung in einem afrikanischen Bauernstaat, 1919–1939," 446
- Serle, Geoffrev, "John Monash: A Biography," 1054
- Seward, Desmond, "The Hundred Years War: The English in France, 1337–1453," 968
- SEX AND SEX ROLES: D'Emilio, "Sexual Politics, Sexual Communities: The Making of a Homosexual Minority in the United States, 1940–1970," 1341; Waller, "Reverend Beecher and Mrs. Tilton: Sex and Class in Victorian America," 484
- "Sexual Politics, Sexual Communities: The Making of a Homosexual Minority in the United States, 1940–1970," by D'Emilio, 1341
- Shade, William G. (R), 1064
- Shaffer, John W., "Family and Farm: Agrarian Change and Household Organization in the Loire Valley, 1500–1900," 686
- Shankman, Arnold, "Ambivalent Friends: Afro-Americans View the Immigrant," 768
- Shannon, Richard, "Gladstone," vol. 1: "1809–1865," 1266
- "The Shaping of Peace: Canada and the Search for World Order, 1943–1957," by Holmes, 220
- "The Shaping of Somali Society," by Cassanelli, 1038
- Sharp, Buchanan (R), 108
- Shaw, William, "Legal Norms in a Confucian State," 455
- Shaw, William H. (R), 1243
- Shedel, James (R), 1244
- Shedel, James, "Art and Society: The New Art Movement in Vienna, 1897–1914," 426
- Sheehan, Bernard W. (R), 746
- Sheehan, James J. (R), 418
- Sheils, W. J., editor, "The Church and Healing" (E), 1355
- Shepelev, L. E., "Tsarizm i burzhuaziia vo vtoroi polovine XIX veka: Problemy torgovo-promyshlennoi politiki," 438
- Sherborne, J. W., and V. J. Scattergood, editors, "English Court Culture in the Later Middle Ages" (E), 1121
- Sheigold, Peter R., "Working-Class Life: The 'American Standard' in Comparative Perspective, 1899–1913," 1089
- Sheridan, Eugene R., "Lewis Morris, 1671–1746: A Study in Early American Politics," 174
- Sheridan, James E. (R), 450
- Sherman, William L. (R), 780
- Shetland, Robert Stewart, Lord of, 989
- Shideler, James H. (R), 1087
- Shils, Edward, "Tradition," 961
- Shintoism 645
- Shipton, Clifford Kenyon, 232, 1124
- Shkurinov, P. S., "Pozitivizm v Rossii XIX veka," 147
- "A Short History of Twentieth-Century Technology," by Williams, 653
- Shorter, Edward (R), 688
- Showalter, Dennis E., "Little Man, What Now? *Der Stürmer* in the Weimar Republic," 706
- Sibley, Marilyn McAdams (R), 1074
- "Sibley's Heir," edited by Allis and Smith (E), 232
- Siewert, Peter, "Die Tritvten Attikas und die Heeresreform des Kleisthenes," 368
- Sikorski, Wladyslaw, 1297
- Silbey, Joel H. (R), 168
- Silverman, Dan P., "Reconstructing Europe after the Great War," 383
- Singal, Daniel Joseph, "The War Within: From Victorian to Modernist Thought in the South, 1919–1945," 772
- Siracusa, Joseph M. (R), 211
- Sironneau, Jean-Pierre, "Sécularisation et religions politiques," 359
- "Sisters or Citizens? Women and Socialism in France since 1876," by Sowerwine, 406
- "Des sittlichen Bürgers Abendschule," by Haider-Pregler, 1284
- Siu, Bobby (R), 1044
- Skeen, C. Edward, "John Armstrong, Jr., 1758–1843: A Biography," 470
- Skinner, A. S., and R. H. Campbell, "Adam Smith," 977
- Skinner, Andrew S., and R. H. Campbell, editors, "The Origins and Nature of the Scottish Enlightenment" (E), 516
- Skotis, Dennis N. (R), 431
- Skocpol, Theda (R), 79
- Skowronek, Stephen (R), 1083
- Slaughter, C. C., 1074
- "Slavers in Paradise: Peruvian Slave Trade in Polynesia, 1862–1864," by Maude, 510
- SLAVERY: Berlin and Gutman, *Natives and Immigrants, Free Men and Slaves: Urban Workingmen in the Antebellum South, 1175–1200*; Curtin, *Laboratory for the Oral History of Slavery: The Island of Lamu on the Kenya Coast*, 858–82; Dockès, "Medieval Slavery and Liberation," 372; Fragnials, Klein, and Engerman, *The Level and Structure of Slave Prices on Cuban Plantations in the Mid-Nineteenth Century: Some Comparative Perspectives, 1201–18*; Helle, "Slavery in Russia, 1450–1725," 1023; Manning, *Contours of Slavery and Social Change in Africa*, 835–57;

- Manning, "Slavery, Colonialism, and Economic Growth in Dahomey, 1640–1969," 727; Maude, "Slavers in Paradise: Peruvian Slave Trade in Polynesia, 1862–1864," 510; Murray, "Odious Commerce," 222; Oakes, "The Ruling Race: A History of American Slaveholders," 1066; Rawley, "The Transatlantic Slave Trade," 361; Russell-Wood, "The Black Man in Slavery and Freedom in Colonial Brazil," 1117; Turner, "Slaves and Missionaries: The Disintegration of Jamaican Slave Society," 1114
- "Slavery and British Society, 1776–1846," edited by Walvin (E), 788
- "Slavery, Colonialism, and Economic Growth in Dahomey, 1640–1960," by Manning, 727
- "Slavery in Russia, 1450–1725," by Hellie, 1023
- "Slavery, War, and Revolution: The British Occupation of Saint Domingue, 1793–1798," by Geggus, 503
- "Slaves and Missionaries: The Disintegration of Jamaican Slave Society," by Turner, 1114
- Slinn, Peter, and Denis Judd, "The Evolution of the Modern Commonwealth, 1902–80," 988
- Smallwood, James (R), 1094
- Smith, Adam, 977
- Smith, Arnold, "Stitches in Time: The Commonwealth in World Politics," 85
- Smith, Bonnie G. (R), 406
- Smith, Bonnie G., "Ladies of the Leisure Class: The Bourgeoises of Northern France in the Nineteenth Century," 405
- Smith, Canfield F. (R), 719
- Smith, Dennis, "Conflict and Compromise: Class Formation in English Society, 1830–1914—A Comparative Study of Birmingham and Sheffield," 982
- Smith, Dennis Mack, "Mussolini," 429
- Smith, F. B., "Florence Nightingale: Reputation and Power," 397
- Smith, Philip Chadwick Foster, and Frederick S. Allis, Jr., editors, "Sibley's Heir: A Volume in Memory of Clifford Kenyon Shipton" (E), 232
- Smith, R. E. F. (R), 1023
- Smith, Robert A. (R), 673
- Smith, Robert Freeman (R), 781
- Smith, Robert J. (R), 128
- Smith, Woodruff (R), 1011
- Sobel, Mechal (R), 476
- "Social Conflict and the Political Order in Modern Britain," edited by Cronin and Schneer (E), 788
- "Social Democracy and Society," by Nolan, 132
- "The Social Gospel in the South: The Woman's Home Mission Movement in the Methodist Episcopal Church," by McDowell, 1084
- SOCIAL HISTORY: Baer, "Fellah and Townsman in the Middle East," 723; Bahmueller, "The National Charity Company," 979; Barnes, "The New Empire of Diocletian and Constantine," 660; Barratt, "Russophobia in New Zealand, 1838–1908," 1313; Beer, "Rebellion and Riot: Popular Disorder in England during the Reign of Edward VI," 670; Brown, "In the Shadow of Florence: Provincial Society in Renaissance Pescia," 1292; Bruns, "Knights of the Road: A Hobo History," 494; Cohen, "La promotion des Juifs en France à l'époque du Second Empire, 1852–1870," 405; Crunden, "Ministers of Reform: The Progressives' Achievement," 762; Daniel, "The Political and Social History of Khurasan under Abassid Rule, 747–820," 149; Ekirch, "'Poor Carolina': Politics and Society in Colonial North Carolina, 1729–1776," 463; Fragnoli, "The Transformation of Reform: Progressivism in Detroit," 1097; Grant, "Self-Help in the 1890s Depression," 1081; Grassl, "Sozialökonomische Vorstellungen in der kaiserzeitlichen griechischen Literatur," 1253; Hampel, "Temperance and Prohibition in Massachusetts, 1813–1852," 1321; Handlin and Handlin, "A Restless People: Americans in Rebellion, 1770–1787," 178; Hane, "Peasants, Rebels, and Outcasts: The Underside of Modern Japan," 157; Hareven, "Family Time and Industrial Time," 486; Holmquist, editor, "They Chose Minnesota: A Survey of the State's Ethnic Groups," 487; Im Hof, "Das gesellige Jahrhundert: Gesellschaft und Gesellschaften im Zeitalter der Aufklärung," 972; Isaac, "The Transformation of Virginia, 1740–1790," 464; Ismael, "Kuwait," 444; Keddie, *The Iranian Revolution in Comparative Perspective*, 579–98; Lindley, "Fenland Riots and the English Revolution," 388; Lorcin, "Vivre et mourir en Lyonnais," 101; McCarthy, "Noblesse Oblige: Charity and Cultural Philanthropy in Chicago, 1849–1929," 483; McManners, "Death and the Enlightenment," 124; Main, "Tobacco Colony: Life in Early Maryland, 1650–1720," 1318; Manning, *Contours of Slavery and Social Change in Africa*, 835–57; Meehan-Waters, "Autocracy and Aristocracy: The Russian Service Elite of 1730," 437; Menefee, "Wives for Sale: An Ethnographic Study of British Popular Divorce," 109; Mintz, "The Anarchists of Casa Vieja," 1276; Mommsen and Mock, editors, "Die Entstehung des Wohlfahrtsstaates in Grossbritannien und Deutschland, 1850–1950," 976; Montias, "Artists and Artisans in Delft," 411; Morgan, "Drugs in America," 169; Mozzarelli, "Sovrano, società e amministrazione locale nella Lombardia teresiana, 1749–1758," 1293; Neuman, "The Speenhamland County: Poverty and the Poor Laws in Berkshire," 1265; Olien and Olien, "Oil Booms: Social Change in Five Texas Towns," 199; Phillips, "Divorce in New Zealand," 164; Saller, "Personal Patronage under the Early Empire," 657; Salmon, "The Making of Roman Italy," 655; Saunders, "A Social History of Black Slaves and Freedmen in Portugal, 1441–1555," 410; Starn, "Contrary Commonwealth: The Theme of Exile in Medieval and Renaissance Italy," 1258; Suny, *Toward a Social History of the October Revolution*, 31–52; Tennstedt, "Sozialgeschichte der Sozialpolitik in Deutschland," 129; Tiflin, "In Whose Best Interest? Child Welfare Reform in the Progressive Era," 762; Tumarkin, "Lenin Lives! The Lenin Cult in Soviet Russia," 1301; Vail and White, *Forms of Resistance: Songs and Perceptions of Power in Colonial Mozambique*, 883–919; Waldmann *et al.*, "Die geheime Dynamik autoritärer Diktaturen: Vier Studien über sozialen Wandel in der Franco-Ära," 1002; Waller, "Reverend Beecher and Mrs. Tilton: Sex and Class in Victorian America," 484; Weinstein and Bell, "Saints and Society: The Two Worlds of Western Christendom, 1000–1700," 1255; Woll, "A Functional Past: The Uses of History in Nineteenth-Century Chile," 512; Zuckerman, editor, "Friends and Neighbors: Group Life in America's First Plural Society," 1319
- "A Social History of Black Slaves and Freedmen in Portugal, 1441–1555," by Saunders, 410
- "The Social Organization of Early Industrial Capitalism," by Katz *et al.*, 1109
- "Social Protest, Violence, and Terror in Nineteenth- and Twentieth-Century Europe," edited by Mommsen and Hirschfeld (E), 1356
- "The Social Sciences," edited by Kruskal (E), 232

- SOCIALISM: Britain, "Fabianism and Culture," 1268;
Geyer, "Kautsky's Russisches Dossier," 1012;
Ghibaudi, "Lavoro e socialismo: Abbozzo di una
storia della concezione socialista del lavoro," 963;
Lipow, "Authoritarian Socialism in America," 194;
Sully, "Continuity and Change in Austrian
Socialism," 1290; Vigezzi, "Il PSI, le riforme e la
rivoluzione," 427
"Società, politica e cultura a Carpi," edited by Avesani
et al. (E), 235
SOCIOLOGY: Banks, "Victorian Values: Secularism and
the Size of Families," 395; Spittler, "Verwaltung in
einem afrikanischen Bauernstaat, 1919–1939," 446
"Socialdemokratyczna partia wolnego miasta Gdańska,
1920–36," by Andrzejewski, 437
Sofaer, Abraham D. (R), 1056
"Un 'soldino' contro il fascismo: Istituzioni ed élites
politiche nella Sicilia del 1923," by Saija, 428
Soliday, Gerald L. (R), 378
Solov'ev, Iu. B., "Samoderzhavie i dvorianstvo v
1902–1907 gg," 1301
Soloway, Richard Allen, "Birth Control and the
Population Question in England, 1877–1930," 680
Solter, Hans Chr., "Adel og embede:
Embedstfordeling og karrieremobilitet hos den
dansk-norske adel, 1588–1660," 1279
Soltow, Lee, and Edward Stevens, "The Rise of
Literacy and the Common School in the United
States: A Socioeconomic Analysis to 1870," 481
Sombart, Nicolaus, and John C. G. Röhl, editors,
"Kaiser Wilhelm II: New Interpretations," 701
Sonnichsen, C. L., "Tucson: The Life and Times of
an American City," 1340
Soroko, Waclaw W. (R), 1022
Sosin, J. M., "English America and the Revolution of
1688: Royal Administration and the Structure of
Provincial Government," 1058
"South African Politics," by Thompson and Prior, 447
Southcott, Joanna, 392
"Southern Anglicanism: The Church of England in
Colonial South Carolina," by Bolton, 1057
"Southern Black Leaders of the Reconstruction Era,"
edited by Rabinowitz, 1071
"Southern Businessmen and Desegregation," edited
by Colburn and Jacoway, 1343
"Southern Honor: Ethics and Behavior in the Old
South," by Wyatt-Brown, 753
"Southwestern Agriculture," edited by Dethloff and
May, 758
"Sovereign States in an Age of Uncertainty," edited by
Hoffman and Albert, 1061
"Soviet Historians in Crisis, 1928–1932," by Barber,
148
"Soviet Policy in East Asia," edited by Zagoria (E), 790
"Il sovrano pontefice—Un corpo e due anime: La
monarchia papale nella prima età moderna," by
Prodi, 1016
"Sovrano, società e amministrazione locale nella
Lombardia teresiana, 1749–1758," by Mozzarelli,
1293
Sowell, Thomas (C), 804
Sowerwine, Charles, "Sisters or Citizens? Women and
Socialism in France since 1876," 406
"Sozialgeschichte der Sozialpolitik in Deutschland," by
Tennstedt, 129
"Sozialgeschichte spanischer Adelskorporationen: Die
Maestranzas de Caballería, 1670–1808," by Liehr,
1001
"Sozialökonomische Vorstellungen in der
kaiserzeitlichen griechischen Literatur," by Grassl,
1253
"Spain, the Jews, and Franco," by Avni, 409
Spalding, Karen, editor, "Essays in the Political,
Economic, and Social History of Colonial Latin
America" (E), 792
"Spanish City Planning in North America," by Crouch
et al., 166
"The Spanish Civil War, 1936–39: American
Hemispheric Perspectives," edited by Falcoff and
Pike, 1353
"Spanish Politics and Imperial Trade, 1700–1789," by
Walker, 223
Spaulding, Robert M. (R), 159
Speck, Paul, "Artabasdos, der rechtgläubige
Vorkämpfer der göttlichen Lehren:
Untersuchungen zur Revolte des Artabasdos und
ihrer Darstellung in der byzantinischen
Historiographie," 94
Speck, W. A., "The Butcher: The Duke of
Cumberland and the Suppression of the 45," 684
Spector, Sherman D. (R), 712
"The Speenhamland County: Poverty and the Poor
Laws in Berkshire," by Neuman, 1265
Spence, Clark C. (R), 757
Spencer, John (R), 152
Spencer, Warren F., "The Confederate Navy in
Europe," 1325
Spielman, John P. (R), 425
Spira, Thomas (R), 1295
Spittler, Gerd, "Verwaltung in einem afrikanischen
Bauernstaat: Das koloniale Französisch-Westafrika,
1919–1939," 446
Spivey, Donald (R), 768
SPORTS AND LEISURE ACTIVITIES: Haug, "Leisure and
Urbanism in Nineteenth-Century Nice," 997;
Murdock, "Ban Johnson: Czar of Baseball," 1095
Spring, D. W., and Nicholas Pronay, editors,
"Propaganda, Politics, and Film, 1918–45" (E), 515
"Staat und Kirche in der Gesellschaft," by Blessing,
1010
"Staat und Synagoge, 1918–1938," by Birnbaum, 422
Stackelberg, Roderick (R), 133
Stadter, Philip A. (R), 367
"Stalinism," edited by Urban (E), 1123
Stănescu, Eugen, and Nicolae-Șerban Tanașoca,
editors, "Études Byzantines et post-Byzantines" (E),
1357
Stanihurst, Richard, 991
Stansky, Peter (R), 678
Stark, Gary D., and Bede Karl Lackner, editors,
"Essays on Culture and Society in Modern
Germany," 1282
Starn, Randolph, "Contrary Commonwealth: The
Theme of Exile in Medieval and Renaissance Italy,"
1258
Starr, Stephen Z., "The Union Cavalry in the Civil
War," vol. 2: "The War in the East from Gettysburg
to Appomattox, 1863–1865," 1069
Start, Gary D. (R), 706
"State Economic Policies of the Ch'ing Government,
1840–1895," by Ch'en, 731
"State-Building in Modern China: The Kuomintang
in the Prewar Period," by Bedeski, 450
"Stato assoluto e società agraria in Prussia nell'età di
Federico II," by Corni, 1285
Stearns, Peter N., editor, "Old Age in Preindustrial
Society" (E), 1357
Stenson, Gary P. (R), 1012
Stein, Burton, "Peasant, State, and Society in Medieval
South India," 161
Steinweg, Reiner, editor, "Hilfe + Handel = Frieden?
Die Bundesrepublik in der Dritten Welt" (E), 789
Stembridge, Stanley R., "Parliament, the Press, and
the Colonies, 1846–1880," 983

- Stenson, Michael, "Class, Race, and Colonialism in West Malaysia: The Indian Case," 1311
- Stephens, Lester D., "Joseph LeConte: Gentle Prophet of Evolution," 1328
- Stern, Steve J., "Peru's Indian Peoples and the Challenge of Spanish Conquest: Huamanga to 1640," 508
- Sternsher, Bernard (R), 495
- Stevens, Edward, and Lee Soltow, "The Rise of Literacy and the Common School in the United States: A Socioeconomic Analysis to 1870," 481
- Stewart, Robert, Earl of Orkney, Lord of Shetland, 989
- Stilgoe, John R., "Common Landscape of America, 1580–1845," 166
- "Stitches in Time: The Commonwealth in World Politics," by Smith, 85
- Stivers, William, "Supremacy and Oil: Iraq, Turkey, and the Anglo-American World Order, 1918–1930," 1270
- Stoianovich, Traian (R), 140
- Stokes, Durward T., "Company Shops: The Town Built by a Railroad," 189
- Stokes, Gale (R), 961
- Storch, Robert D., editor, "Popular Culture and Custom in Nineteenth-Century England" (E), 515
- Storing, Herbert J., editor, "The Complete Anti-Federalist," vol. 1: "What the Anti-Federalists Were For"; vol. 2: "Objections of Non-Signers of the Constitution and Major Series of Essays at the Outset"; vol. 3: "Pennsylvania"; vol. 4: "Massachusetts"; vol. 5: "Maryland and Virginia and the South"; vol. 6: "New York and Conclusion"; vol. 7: "Index" compiled by John Robinson, 467
- Stover, John F. (R), 1086
- Stow, George B. (R), 102
- Stozier, Charles B., "Lincoln's Quest for Union: Public and Private Meanings," 1070
- Stranges, Anthony N., "Electrons and Valence: Development of the Theory, 1900–1925," 964
- Streicher, Julius, 706
- Stromberg, Roland (R), 998, 1245
- Stromberg, Roland N., "Redemption by War: The Intellectuals and 1914," 105
- "Structure and Change in Economic History," by North, 360
- "The Structure of Nineteenth-Century Cities," edited by Johnson and Pooley (E), 787
- "The Struggle over Eritrea, 1962–1978," by Erlich, 1305
- Struminger, Laura S., "What Were Little Girls and Boys Made of? Primary Education in Rural France, 1830–1880," 1273
- Struve, Walter (R), 702, 1282
- Stuart, Reginald C., "War and American Thought from the Revolution to the Monroe Doctrine," 752
- "Students, Society, and Politics in Imperial Germany," by Jarausch, 702
- Stueck, William (C), 527
- Sullivan, Robert E., "John Toland and the Deist Controversy: A Study in Adaptations," 389
- Sullivan, Walter, and William C. Havard, editors, "A Band of Prophets: The Vanderbilt Agrarians after Fifty Years" (E), 235
- Sully, Melanie A., "Continuity and Change in Austrian Socialism: The Eternal Quest for the Third Way," 1290
- Summers, Robert Samuel, "Instrumentalism and American Legal Theory," 1335
- Sundhaussen, Ulf, "The Road to Power: Indonesian Military Politics, 1945–1967," 740
- "Sundsvallsdistriktets sågverksarbetare, 1860–1890," by Cornell, 412
- Sunseri, Alvin R. (R), 1068
- Suny, Ronald Grigor (C), 1139
- Suny, Ronald Grigor, *Toward a Social History of the October Revolution*, 31–52
- "Suomalainen äärioikeisto maailmansotien välillä," by Nygård, 1280
- "Supremacy and Oil: Iraq, Turkey, and the Anglo-American World Order, 1918–1930," by Stivers, 1270
- Sussman, George D. (R), 996
- Sussman, George D., "Selling Mothers' Milk: The Wet-Nursing Business in France, 1715–1914," 688
- Sutcliffe, Anthony (R), 1267
- Sutherland, Donald W. (R), 373
- Swann, Michael M., "Tierra Adentro: Settlement and Society in Colonial Durango," 1115
- Sweet, Paul R. (R), 1283
- Swenson, Loyd S., Jr. (R), 964
- Swinton, Philip Cunliffe-Lister, Lord, 1271
- "Synesius of Cyrene," by Bregman, 369
- Sysyn, Frank S., and Andrei S. Markovits, editors, "Nationbuilding and the Politics of Nationalism: Essays on Austrian Galicia" (E), 1122
- Szeftel, Marc (R), 666
- Szilassy, Sandor (R), 715
- Szporluk, Roman, "The Political Thought of Thomas G. Masaryk," 713
- "T. F. Wade in China," by Cooley, 1043
- Tackett, Timothy (R), 124
- Tales of Two Cities: Florence and Venice in the Renaissance* (Review Essay), by Brucker, 599–616
- Tallgren, Vappu, "Hitler und die Helden: Heroismus und Weltanschauung," 133
- Tanaşoca, Nicolae-Şerban, and Eugen Stănescu, editors, "Études Byzantines et post-Byzantines" (E), 1357
- Tanenbaum, Jan Karl (R), 711
- "Tanzania: A Political Economy," by Coulson, 1040
- Tariello, Frank, Jr., "The Reconstruction of American Political Ideology, 1865–1917," 492
- Tarr, Joel A. (R), 489
- Taylor, Alan R., "The Arab Balance of Power," 725
- Taylor, Joe Grey (R), 473
- Taylor, Robert J., *et al.*, "Diary of John Quincy Adams," vol. 1: "November 1779–March 1786"; vol. 2: "March 1786–December 1788; Index," 179
- Taylor, Rodney L. (R), 153
- Taylor, Romeyn (R), 449
- TECHNOLOGY: Allison, "New Eye for the Navy: The Origin of Radar," 210; McNeill, "The Pursuit of Power: Technology, Armed Force, and Society since A.D. 1000," 1239; Williams, "A Short History of Twentieth-Century Technology," 653
- "Télévision, nouvelle mémoire: Les magazines de grand reportage, 1959–1968," edited by Jeanneney and Sauvage, 1275
- Temkin, S. D. (R), 672
- "Temperance and Prohibition in Massachusetts, 1813–1852," by Hampel, 1321
- Tennstedt, Florian, "Sozialgeschichte der Sozialpolitik in Deutschland: Vom 18. Jahrhundert bis zum Ersten Weltkrieg," 129
- Tennyson, Brian Douglas, "Canadian Relations with South Africa: A Diplomatic History," 1112
- "Terminal Visions," by Wagar, 962
- Terrill, Tom E. (R), 771
- Terry, Sarah Meiklejohn, "Poland's Place in Europe:

- General Sikorski and the Origin of the Oder-Neisse Line, 1939–1943," 1297
- "La Terza Internazionale e il fascismo, 1919–1923," by Natoli, 384
- "Teso in Transformation," by Vincent, 1305
- THEATRE: Haider-Pregler, "Des sittlichen Bürgers Abendschule," 1284
- "Thebes in the Fifth Century," by Demand, 1251
- Theoharis, Athan G., editor, "Beyond the Hiss Case: The FBI, Congress, and the Cold War," 1105
- "A Theory of History," by Heller, 358
- "They Chose Minnesota: A Survey of the State's Ethnic Groups," edited by Holmquist, 487
- "The Thirty at Athens," by Krentz, 654
- Thomas, C. G. (R), 1251
- Thomas, Carol G., and Robert Griffith, editors, "The City-State in Five Cultures," 81
- Thomas, Charles, "Christianity in Roman Britain to A.D. 500," 967
- Thomas, John L., "Alternative America: Henry George, Edward Bellamy, Henry Demarest Lloyd, and the Adversary Tradition," 1330
- Thomas, William (R), 981
- Thompson, David M. (R), 983
- Thompson, Dorothy, and James Epstein, editors, "The Chartist Experience: Studies in Working-Class Radicalism and Culture, 1830–60" (E), 1121
- Thompson, F. M. L., editor, "The Rise of Suburbia," 680
- Thompson, I. A. A. (R), 1001
- Thompson, James J., Jr., "Tried as by Fire: Southern Baptists and the Religious Controversies of the 1920s," 1096
- Thompson, John M. (R), 1301
- Thompson, Leonard, and Andrew Prior, "South African Politics," 447
- Thompson, Leonard, and Howard Lamar, editors, "The Frontier in History: North America and Southern Africa Compared," 84
- Thompson, Paul, editor, "Our Common History: The Transformation of Europe" (E), 1356
- Thompson, Roger C. (R), 1313
- Thomson, J. K. J., "Clermont-de-Lodève, 1633–1789: Fluctuations in the Prosperity of a Languedocian Cloth-Making Town," 993
- Thornton, J. Mills, III (R), 753
- Thorpe, Frederick J., "Ramparts lointains: La politique française des travaux publics à Terre-Neuve et à l'Île Royale, 1695–1758," 219
- Tiedemann, Arthur E. (R), 158
- "Tierra Adentro," by Swann, 1115
- Tiffin, Susan, "In Whose Best Interest? Child Welfare Reform in the Progressive Era," 762
- Tiller, Veronica E. Velarde, "The Jicarilla Apache Tribe: A History, 1846–1970," 1332
- Tillman, Hoyt Cleveland, "Utilitarian Confucianism: Ch'en Liang's Challenge to Chu Hsi," 153
- Tilly, Charles (R), 1241
- Tilly, Louise A. (R), 104
- Tipton, C. Leon (R), 95
- Tipton, Elise K. (C), 526
- Tipton, Frank B. (R), 975
- "Die Tiroler Frage auf der Friedenskonferenz von Saint Germain," by Schober, 1290
- Tirro, Frank, editor, "Medieval and Renaissance Studies" (E), 233
- "Tithe and Agrarian History from the Fourteenth to the Nineteenth Centuries," by Le Roy Ladurie and Goy, 1240
- "To Free a People: American Jewish Leaders and the Jewish Problem in Eastern Europe, 1890–1914," by Best, 198
- "Tobacco Colony: Life in Early Maryland, 1650–1720," by Main, 1318
- Tobias, Marilyn, "Old Dartmouth on Trial: The Transformation of the Academic Community in Nineteenth-Century America," 760
- Tobriner, Stephen, "The Genesis of Noto: An Eighteenth-Century Sicilian City," 1018
- Tokody, Gyula, "Deutschland und die ungarische Räterepublik," 1295
- Toland, John (1670–1722), 389
- Toland, John, 958–60
- Toland, John, "Infamy: Pearl Harbor and Its Aftermath," 496
- Toll, William, "The Making of an Ethnic Middle Class: Portland Jewry over Four Generations," 769
- Tollison, Robert D., and Robert B. Ekelund, Jr., "Mercantilism as a Rent-Seeking Society: Economic Regulation in Historical Perspective," 81
- Tombs, Robert, "The War against Paris, 1871," 127
- Tomisch, John (R), 193
- Toniolo, Gianni, editor, "L'economia italiana, 1861–1940," 1293
- "The Tontine from the Reign of Louis XIV to the French Revolutionary Era," by Jennings and Trout, 1272
- Touhill, Blanche M. (R), 401
- "Toward Increased Judicial Activism," by Miller, 1107
- "Toward a Programme of Imperial Life: The British Empire," by Field, 667
- Toward a Social History of the October Revolution*, by Suny, 31–52
- Trachtenberg, Alan, "The Incorporation of America: Culture and Society in the Gilded Age," 193
- "Tradition," by Shils, 961
- Traer, James F. (R), 125, 689
- Trafzer, Clifford E., "The Kit Carson Campaign: The Last Great Navajo War," 479
- "The Transatlantic Slave Trade," by Rawley, 361
- "The Transfer of Power in Africa," edited by Gifford and Louis (E), 790
- "The Transformation of Intellectual Life in Victorian England," by Heyck, 981
- "The Transformation of Reform: Progressivism in Detroit," by Fragnoli, 1097
- "The Transformation of Virginia, 1740–1790," by Isaac, 464
- "Transforming Russia and China," by Rosenberg and Young, 79
- TRANSPORTATION: Bardou *et al.*, *The Automobile Revolution*, 86
- Treadgold, Warren T. (R), 94
- Treadway, John D. (R), 1019
- Trebilcock, Clive, "The Industrialization of the Continental Powers, 1780–1914," 974
- Trefousse, Hans L. (R), 1073
- Trefousse, Hans L., "Carl Schurz: A Biography," 474
- Treggiari, Susan (R), 1254
- "Tres aspectos de la presencia española en Mexico," by Miño Grijalva *et al.*, 226
- Trevor-Roper, Hugh R., 231
- "Tribalism in Crisis: Federal Indian Policy, 1953–1961," by Burt, 1107
- "Tried as by Fire: Southern Baptists and the Religious Controversies of the 1920s," by Thompson, 1096
- Trinder, Barrie, "The Making of the Industrial Landscape," 1264
- "Trinity College, Dublin, 1592–1952," by McDowell and Webb, 684
- Trinkaus, Charles (R), 646
- Tripathi, Dwijendra, "The Dynamics of a Tradition: Kasturbhai Lalbhai and His Entrepreneurship," 1051

- Triska, Jan F., and Charles Gati, editors, "Blue-Collar Workers in Eastern Europe," 140
- "Die Trittitten Atikas und die Heeresreform des Kleisthenes," by Siewert, 368
- "Trois études sur l'Arabie romaine et byzantine," by Sartre, 370
- Trotsky, Leon, 79
- Trout, Andrew (R), 123
- Trout, Andrew P., and Robert M. Jennings, "The Tontine from the Reign of Louis XIV to the French Revolutionary Era," 1272
- Truman, Harry S., 1344
- "Die Truppe des Weltanschauungskrieges," by Krausnick and Wilhelm, 422
- "Tsarizm i burzhuaizii vo vtoroi polovine XIX veka," by Shepelev, 438
- "Tsarizm i IV Duma, 1912–1914 gg.," by Avrek, 1029
- Tucci, Giuseppe, "The Religions of Tibet," 1309
- Tuck, Jim, "The Holy War in Los Altos: A Regional Analysis of Mexico's *Cristero* Rebellion," 1116
- Tucker, Richard P., and J. F. Richards, editors, "Global Deforestation and the Nineteenth-Century World Economy" (E), 1120
- Tucker, Robert W., and David C. Hendrickson, "The Fall of the First British Empire: Origins of the War of American Independence," 1059
- Tull, Charles J. (R), 494
- "Tucson," by Sonnichsen, 1340
- Tumarkin, Nina, "Lenin Lives! The Lenin Cult in Soviet Russia," 1301
- Turati, Filippo, 427
- Turner, Henry A. (C), 1143
- Turner, Mary, "Slaves and Missionaries: The Disintegration of Jamaican Slave Society, 1787–1834," 1114
- Turner, Michael, "The Vice President as Policy Maker: Rockefeller in the Ford White House," 1347
- Turner, Thomas R. (R), 754
- Turner, Thomas Reed, "Beware the People Weeping: Public Opinion and the Assassination of Abraham Lincoln," 1070
- "The Turning Point in Africa: British Colonial Policy, 1938–48," by Pearce, 445
- Tuttle, Richard J. (R), 1018
- "TVA and the Dispossessed: The Resettlement of Population in the Norris Dam Area," by McDonald and Muldowny, 1341
- "1204," by Godfrey, 95
- Tylor, E. B., 649
- "Typhus and Doughboys," by Corneise, 1335
- Tyrell, Ian (R), 1321
- Leff, 782
- Unger, Jonathan, "Education under Mao: Class and Competition in Canton Schools, 1960–1980," 452
- "The Union Cavalry in the Civil War," vol. 2: "The War in the East from Gettysburg to Appomattox," by Starr, 1069
- "United States Foreign Policy at the Crossroads," edited by Schwab, 1358
- Unrau, William E. (R), 1078
- "The Upstart Earl: A Study of the Social and Mental World of Richard Boyle," by Canny, 992
- Upton, Anthony F. (R), 697
- "The Urban Establishment," by Jaher, 167
- URBAN HISTORY: Berlin and Gutman, *Natives and Immigrants, Free Men and Slaves: Urban Workingmen in the Antebellum South*, 1175–1200; Cohen, "The Politics of Ethnic Survival: Germans in Prague, 1871–1914," 434; Corfield, "The Impact of English Towns, 1700–1800," 676; Crouch *et al.*, "Spanish City Planning in North America," 166; François, "Koblenz im 18. Jahrhundert," 700; Fraser, editor, "Municipal Reform and the Industrial City," 680; Garside and Young, "Metropolitan London: Politics and Urban Change, 1837–1981," 1267; Gottfried, "Bury St. Edmunds and the Urban Crisis, 1290–1539," 375; Kahrl, "Water and Power: The Conflict over Los Angeles' Water Supply in the Owens Valley," 201; Leavitt, "The Healthiest City: Milwaukee and the Politics of Health Reform," 488; McCarthy, "Noblesse Oblige: Charity and Cultural Philanthropy in Chicago, 1849–1929," 483; McClain, "Kanazawa: A Seventeenth-Century Japanese Castle Town," 1307; Miner, "Wichita: The Early Years, 1865–80," 1327; Moehring, "Public Works and the Patterns of Urban Real Estate Growth in Manhattan, 1835–1894," 489; Moritz, "Die bürgerlichen Fürsorgeanstalten der Reichsstadt Frankfurt a.M. im späten Mittelalter," 377; Noel, "The City and the Saloon," 757; O'Brien, "'Dear, Dirty Dublin': A City in Distress, 1899–1916," 685; Schaffer, "Garden Cities for America: The Radburn Experience," 770; Sonnichsen, "Tucson," 1340; Stilgoe, "Common Landscape of America, 1580–1845," 166; Stokes, "Company Shops: The Town Built by a Railroad," 189; Thompson, editor, "The Rise of Suburbia," 680; Tobriner, "The Genesis of Noto," 1018; Waller, "Democracy and Sectarianism: A Political and Social History of Liverpool, 1868–1939," 398; Western, "Outcast Cape Town," 731
- Urban, G. R., editor, "Stalinism: Its Impact on Russia and the World" (E), 1123
- Urban, William, "The Livonian Crusade," 96
- Urban, William, "The Prussian Crusade," 96
- Uschakow, Alexander, editor, "Polen—Das Ende der Erneuerung? Gesellschaft, Wirtschaft und Kultur im Wandel" (E), 790
- "Utilitarian Confucianism," by Tillman, 153
- "Utopia y reformismo en la España de los Austrias," by Maravall, 1000
- Vail, Leroy, and Landeg White, *Forms of Resistance: Songs and Perceptions of Power in Colonial Mozambique*, 883–919
- Valentić, Mirko, "Vojna krajina i pitanje njezina sjedinjenja s Hrvatskom, 1849–1881," 432
- Van Cauwenberghe, Eddy, "Het vorstelijk domein en de overheidsfinanciën in de Nederlanden, 15de en 16de eeuw: Een kwantitatieve analyse van Vlaamse en Brabantse domeinrekeningen," 1277

- Van Creveld, Martin, "Fighting Power: German and U.S. Army Performance, 1939–1945," 1287
- Van der Dussen, W. J., "History as a Science: The Philosophy of R. G. Collingwood," 73
- Van der Kroef, Justus M. (R), 740
- Van der Stroom, Gerrold, "Duitse strafrechtspleging in Nederland en het lot der veroordeelden," 1004
- Van Kley, Edwin J. (R), 1042
- Van Young, Eric (R), 1350
- "The Vanishing American: White Attitudes and U.S. Indian Policy," by Dippie, 744
- "Vapaustaistelu, kansalaissota ja kapina: Taistelun luonne valkoisten sotapropagandassa vuonna 1918," by Manninen, 697
- "The Vatican and Hungary, 1846–1878," by Lukács, 1295
- "The Vatican and the American Hierarchy from 1870 to 1965," by Fogarty, 1333
- "Vatican I et les évêques uniates," by Patelos, 431
- Vaughn, William Preston, "The Anti-Masonic Party in the United States, 1826–1843," 1064
- Vázquez, Josefina Zoraida, *et al.*, "Ensayos sobre historia de la educación en México," 506
- "Venezuela," by Lombardi, 782
- Vergennes, Charles Gravier, Comte de, 996
- Vernassa, Maurizio, "Emigrazione, diplomazia et cannoniere: L'intervento italiano in Venezuela, 1902–1903," 227
- Vernon, Betty D., "Ellen Wilkinson, 1891–1947," 120
- "Verwaltung in einem afrikanischen Bauernstaat," by Spittler, 446
- "The Vice President as Policy Maker: Rockefeller in the Ford White House," by Turner, 1347
- "Victims, Authority, and Terror," by Kelly, 125
- "Victorian Values: Secularism and the Size of Families," by Banks, 395
- "Vietnamese Communism, 1925–1945," by Khanh, 459
- Vigezzi, Brunello, "Il PSI, le riforme e la rivoluzione: Filippo Turati e Anna Kuliscioff dai fatti del 1898 alla prima guerra mondiale," 427
- "Village and Seaport: Migration and Society in Eighteenth-Century Massachusetts," by Jones, 174
- "La Ville Libre de Dantzig en crise ouverte 24.10.1938–1.9.1939," by Carls-Maire, 1022
- "Vin, vigne, et vigneronns en région parisienne du XVII^e au XIX^e siècle," by Lachiver, 995
- Vincent, Charles (R), 1071
- Vincent, Joan, "Teso in Transformation: The Political Economy of Peasant and Class in Eastern Africa," 1305
- "Vingt-cinq ans d'études dynastiques," by Pinoteau, 993
- Vinten-Johansen, Peter (R), 1279
- Vital, David (R), 972
- Vital, David, "Zionism: The Formative Years," 80
- "Vivre et mourir en Lyonnais," by Lorcin, 101
- Voegelin, Eric, 514
- "Voices of Protest: Huey Long, Father Coughlin, and the Great Depression," by Brinkley, 494
- "Vojna krajina i pitanje njezina sjedinjenja s Hrvatskom, 1849–1881," by Valentić, 432
- Volberg, Heinrich, "Auslandsdeutschum und Drittes Reich: Der Fall Argentinien," 230
- Voll, John Obert, "Islam: Continuity and Change in the Modern World," 1030
- "Het vorstelijk domein en de overheidsfinanciën in de Nederlanden, 15de en 16de eeuw," by Van Cauwenberghe, 1277
- Voss, Stuart F., "On the Periphery of Nineteenth-Century Mexico: Sonora and Sinaloa, 1810–1877," 505
- Vucinich, Alexander (R), 147
- Wade, Thomas Francis, 1043
- Wagar, W. Warren (R), 359
- Wagar, W. Warren, "Terminal Visions: The Literature of Last Things," 962
- Wagar, W. Warren, editor, "The Secular Mind: Transformations of Faith in Modern Europe" (E), 787
- Wagenknecht, Edward, "American Profile, 1900–1909," 490
- Wagner, Jonathan F., "Brothers beyond the Sea: National Socialism in Canada," 220
- Wakelyn, Jon L. (R), 772
- Waldmann, Peter, *et al.*, "Die geheime Dynamik autoritärer Diktaturen: Vier Studien über sozialen Wandel in der Franco-Ära," 1002
- "Waldo Emerson," by Allen, 182
- Walker, Clarence E., "A Rock in a Weary Land: The African Methodist Episcopal Church during the Civil War and Reconstruction," 476
- Walker, Geoffrey J., "Spanish Politics and Imperial Trade, 1700–1789," 223
- Walker, Lawrence D. (R), 1288
- Walkowitz, Daniel J., and Michael H. Frisch, editors, "Working-Class America: Essays on Labor, Community, and American Society" (E), 1124
- Wallace, Dewey D., Jr., "Puritans and Predestination: Grace in English Protestant Theology, 1525–1695," 388
- Wallace, Martin, "British Government in Northern Ireland: From Devolution to Direct Rule," 121
- Waller, Altina L., "Reverend Beecher and Mrs. Tilton: Sex and Class in Victorian America," 484
- Waller, P. J., "Democracy and Sectarianism: A Political and Social History of Liverpool, 1868–1939," 398
- Wallerstein, Immanuel, "The Modern World-System," vol. 2: "Mercantilism and the Consolidation of the European World-Economy, 1600–1750," 81
- "Wall-to-Wall America: A Cultural History of Post-Office Murals in the Great Depression," by Marling, 775
- Waltner, Ann (R), 154
- Walvin, James, editor, "Slavery and British Society, 1776–1846" (E), 788
- Walvin, James, and Edward Royle, "English Radicals and Reformers, 1760–1848," 979
- Wandycz, Piotr S. (R), 436
- "The War against Paris, 1871," by Tombs, 127
- "War against War: British and German Radical Movements in the First World War," by Carsten, 382
- "War and American Thought from the Revolution to the Monroe Doctrine," by Stuart, 752
- "War and Society in Revolutionary Europe, 1770–1870," by Best, 973
- "War and the State: The Transformation of British Government, 1914–1919," edited by Burk (E), 515
- "The War Within: From Victorian to Modernist Thought in the South," by Singal, 772
- Ward, Benedicta, "Miracles and the Medieval Mind: Theory, Record, and Event, 1000–1215," 967
- Ward, J. T. (R), 990
- Warloski, Ronald A. (R), 419
- Warmbrunn, Werner (R), 1004
- Warren, Earl, 777
- "Wars and Revolutions: Britain, 1760–1815," by Christie, 675
- The Wars of August: Diagonal Narratives in African*

- History*, by Harms, 809–34
 “The Wars of the Roses,” by Gillingham, 661
 “The Warsaw Pact,” edited by Kaplan and Clawson (E), 233
 “Water and Power: The Conflict over Los Angeles’ Water Supply in the Owens Valley,” by Kahrl, 201
 Watson, Alan, “The Making of the Civil Law,” 83
 Watson, Harry L., “Jacksonian Politics and Community Conflict: The Emergence of the Second American Party System in Cumberland County, North Carolina,” 181
 Watts, Pauline Moffitt, “Nicolaus Cusanus: A Fifteenth-Century Vision of Man,” 970
 Weare, Walter B. (R), 1343
 Webb, Beatrice, 984
 Webb, D. A., and R. B. McDowell, “Trinity College, Dublin, 1592–1952: An Academic History,” 684
 Weber, David J., “The Mexican Frontier, 1821–1846: The American Southwest under Mexico,” 504
 Webster, Graham, “Rome against Caratacus: The Roman Campaigns in Britain, A.D. 48–58,” 89
 Webster, Richard A. (R), 227
 Wegner, Bernd, “Hitlers Politische Soldaten: Die Waffen-SS, 1933–1945; Studien zu Leitbild, Struktur und Funktion einer nationalsozialistischen Elite,” 1287
 Weigley, Russell F. (R), 1093
 “Weimar Etudes,” by Pachter (E), 1122
 “Weimar Germany: Writers and Politics,” edited by Bance (E), 1122
 Weinberg, Gerhard L. (R), 421
 Weinert, Richard S., and Sylvia Ann Hewlett, editors, “Brazil and Mexico: Patterns in Late Development” (E), 1125
 Weingartner, James J. (R), 1287
 Weinstein, Donald, and Rudolph M. Bell, “Saints and Society: The Two Worlds of Western Christendom, 1000–1700,” 1255
 Weinstein, Fred (R), 1238
 Weiss, John Hubbel, “The Making of Technological Man: The Social Origins of French Engineering Education,” 127
 Weisser, Henry (R), 393
 Wells, Audrey, and F. S. Northedge, “British and Soviet Communism: The Impact of a Revolution,” 682
 Wells, Robert V., “Revolutions in Americans’ Lives: A Demographic Perspective on the History of Americans, Their Families, and Their Society,” 743
 Wells, Robert V., and Manfred Jonas, editors, “New Opportunities in a New Nation: The Development of New York after the Revolution,” 1061
 “Weltpolitik als Kulturmission: Auswärtige Kulturpolitik und Bildungsbürgertum in Deutschland am Vorabend des Ersten Weltkrieges,” by Bruch, 1013
 “Weltrevolution und Völkerbund: Die schweizerische Aussenpolitik,” by Kunz, 136
 Wemple, Suzanne F. (R), 370
 Werner, Karl Ferdinand, and Klaus Hildebrand, editors, “Deutschland und Frankreich, 1936–1939” (E), 788
 Wesley, John, 12–30
 “Westering Women and the Frontier Experience, 1800–1915,” by Myres, 1079
 Western, John, “Outcast Cape Town,” 731
 “Die Westmächte und das Dritte Reich, 1933–1939,” edited by Rohe, 1286
 Westoby, Adam (R), 362
 Wette, Wolfram, and Karl Holl, editors, “Pazifismus in der Weimarer Republik: Beiträge zur historischen Friedensforschung” (E), 234
 “The Whale’s Wake,” by Morton, 1312
 Whaley, Joachim, editor, “Mirrors of Mortality: Studies in the Social History of Death” (E), 514
 “What Were Little Girls and Boys Made of? Primary Education in Rural France, 1830–1880,” by Struminger, 1273
 “Which Side Were You On? The American Communist Party during the Second World War,” by Isserman, 776
 White, G. Edward, “Earl Warren: A Public Life,” 777
 White, Gerald T. (R), 1343
 White, Landeg, and Leroy Vail, *Forms of Resistance: Songs and Perceptions of Power in Colonial Mozambique*, 883–919
 “White Servitude in Colonial America,” by Galenson, 171
 Whitelock, Dorothy, *et al.*, editors, “Ireland in Early Mediaeval Europe: Studies in Memory of Kathleen Hughes” (E), 787
Who Composed ‘Sazonov’s Thirteen Points’? A Re-Examination of Russia’s War Aims of 1914 (Research Note), by Renzi, 347–57;
 “Who Voted for Hitler?” by Hamilton, 705
 “Who’s Who in Nazi Germany,” by Wistrich, 704
 Whorton, James C., “Crusaders for Fitness: The History of American Health Reformers,” 1066
 Whyte, Ian D. (R), 376
 Wiarda, Howard J., and Michael J. Kryzanek, “The Dominican Republic: A Caribbean Crucible,” 503
 Wiatrowski, Michael D. (R), 190
 “Wichita: The Early Years, 1865–80,” by Miner, 1327
 Wickins, P. L., “An Economic History of Africa from the Earliest Times to Partition,” 1034
 Wiecek, William M., and Harold D. Hyman, “Equal Justice under Law: Constitutional Development, 1835–1875,” 469
 Wiener, Joel H. (R), 113
 Wilde, Richard H. (R), 117
 Wilhelm II, 700, 701
 Wilhelm, Hans-Heinrich, and Helmut Krausnick, “Die Truppe des Weltanschauungskrieges: Die Einsatzgruppen der Sicherheitspolizei und des SD, 1938–1942,” Part 1, “Die Einsatzgruppen vom Anschluss Österreichs bis zum Feldzug gegen die Sowjetunion: Entwicklung und Verhältnis zur Wehrmacht”; Part 2, “Die Einsatzgruppe A der Sicherheitspolizei und des SD, 1941/2: Eine exemplarische Studie,” 422
 “Wilhelm Liebknecht and the Founding of the German Social Democratic Party,” by Dominick, 1012
 Wilkinson, Ellen, 120
 Willett, John, “Art and Politics in the Weimar Period: The New Sobriety, 1917–1933,” 420
 Williams, B. R., and W. H. Oliver, editors, “The Oxford History of New Zealand,” 163
 Williams, Edgar, 515
 Williams, Glyndwr, and P. J. Marshall, “The Great Map of Mankind: Perceptions of New Worlds in the Age of Enlightenment,” 971
 Williams, Trevor L., “A Short History of Twentieth-Century Technology, c. 1900–c. 1950,” 653
 Williams, Walter L. (R), 1341
 Willingham, William F. (R), 174
 Wills, John E., Jr. (R), 1052
 Wilson, David Scofield (R), 175
 Wilson, George Macklin (R), 734
 Wilson, Stephen, “Ideology and Experience: Antisemitism in France at the Time of the Dreyfus Affair,” 691
 Wilson, Woodrow, 765, 1091
 Wilz, John Edward (R), 1105

- Winkler, Henry R. (R), 682
 Wistrich, Robert, "Who's Who in Nazi Germany," 704
 "The Witches' Advocate: Basque Witchcraft and the Spanish Inquisition," by Henningsen, 692
 Witek, John W., "Controversial Ideas in China and in Europe: A Biography of Jean-François Fouquet, S. J., 1665-1741," 1042
 "With Good Intentions: Quaker Work among the Pawnees, Otos, and Omahas," by Milner, 1078
 "With Shield and Sword: American Military Affairs, Colonial Times to the Present," by Hassler, 1315
 Wittner, Lawrence S. (R), 212
 Wittner, Lawrence S., "American Intervention in Greece, 1943-1949," 213
 "Wives for Sale: An Ethnographic Study of British Popular Divorce," by Menefee, 109
 Wohl, Anthony S. (R), 685
 Wolfe, Martin (R), 651, 974
 Wolfe, Willard (R), 1268
 Woll, Allen, "A Functional Past: The Uses of History in Nineteenth-Century Chile," 512
 Woloch, Isser (R), 404
 Wolters, Raymond (R), 1342
 "A Woman to Deliver Her People: Joanna Southcott and English Millenarianism in an Era of Revolution," by Hopkins, 392
 "Woman's Legacy: Essays on Race, Sex, and Class in American History," by Aptheker, 1056
 "Women in Turkish Society," edited by Abadan-Unat *et al.*, 1031
 "The Women of England from Anglo-Saxon Times to the Present," edited by Kanner, 1262
 "Women Scientists in America," by Rossiter, 1339
 "Women's Diaries of the Westward Journey," by Schlissel, 184
 WOMEN'S HISTORY: Abadan-Unat *et al.*, editors
 "Women in Turkish Society," 1031; Aptheker, "Woman's Legacy: Essays on Race, Sex, and Class in American History," 1056; Banks, "Faces of Feminism," 83; Basch, "In the Eyes of the Law: Women, Marriage, and Property in Nineteenth-Century America," 485; Gronewold, "Beautiful Merchandise," 1044; Hayden, "The Grand Domestic Revolution: A History of Feminist Designs for American Homes, Neighborhoods, and Cities," 1080; Hume, "The National Union of Women's Suffrage Societies, 1897-1914," 986; Kanner, editor, "The Women of England from Anglo-Saxon Times to the Present," 1262; McDowell, "The Social Gospel in the South: The Woman's Home Mission Movement in the Methodist Episcopal Church," 1084; Macías, "Against All Odds: The Feminist Movement in Mexico to 1940," 507; Marsh, "Anarchist Women, 1870-1920," 1330; Myres, "Westering Women and the Frontier Experience, 1800-1915," 1079; Norton, "Liberty's Daughters: The Revolutionary Experience of American Women," 750; Robertson, "An Experience of Women," 104; Rosen, "The Lost Sisterhood: Prostitution in America, 1900-1918," 1084; Rosenberg, "Beyond Separate Spheres: Intellectual Roots of Modern Feminism," 484; Rossiter, "Women Scientists in America," 1339; Schlissel, "Women's Diaries of the Westward Journey," 184; Smith, "Ladies of the Leisure Class: The Bourgeoises of Northern France in the Nineteenth Century," 405; Sowerwine, "Sisters or Citizens? Women and Socialism in France since 1876," 406; Sussman, "Selling Mothers' Milk: The Wet-Nursing Business in France, 1715-1814," 688; Ulrich, "Good Wives: Image and Reality in the Lives of Women in Northern New England, 1650-1750," 173
 Wood, Douglas Kellogg, "Men against Time: Nicholas Berdyaev, T. S. Eliot, Aldous Huxley, and C. G. Jung," 1245
 "Woodrow Wilson and a Revolutionary World, 1913-1921," edited by Link, 765
 Woodruff, William (R), 81
 Woodward, C. Vann (R), 187
 Woodward, C. Vann, 476
 Woolrych, Austin, "Commonwealth to Protectorate," 672
 Worcester, Donald E. (R), 1332
 "Workers in Bondage: The Origins and Bases of Unfree Labour in Queensland," by Saunders, 741
 "Working-Class America," edited by Frisch and Walkowitz (E), 1124
 "Working-Class Life: The 'American Standard' in Comparative Perspective, 1899-1913," by Shergold, 1089
 WORLD WAR I: Brook-Shepherd, "November 1918," 382; Carsten, "War against War: British and German Radical Movements in the First World War," 382; French, "British Economic and Strategic Planning, 1905-1915," 118; Golczewski, "Der Balkan in deutschen und österreichischen Reise- und Erlebnisberichten, 1912-1918," 1294; Michel, "L'appel à l'Afrique," 728; Mitrakos, "France in Greece during World War I," 711; Monticone, "Deutschland und die Neutralität Italiens, 1914-1915," 1014; Morrow, "German Air Power in World War I," 1014; Morton, "A Peculiar Kind of Politics: Canada's Overseas Ministry in the First World War," 1112; Renzi, *Who Composed 'Sazonov's Thirteen Points'? A Re-Examination of Russia's War Aims of 1914* (Research Note), 347-57; Stromberg, "Redemption by War," 105
 WORLD WAR II: Bratzel and Rout, *Once Again: Pearl Harbor, Microdots, and J. Edgar Hoover* (Research Note), 953-60; Carls-Maire, "La Ville Libre de Dantzig en crise ouverte 24.10.1938-1.9.1939," 1022; Hanson, "The Civilian Population and the Warsaw Uprising of 1944," 1022; Hauner, "India in Axis Strategy," 739; Holmes, "The Shaping of Peace: Canada and the Search for World Order, 1943-1957," 220; Isserman, "Which Side Were You On? The American Communist Party during the Second World War," 776; Jacobson, *Is There a New International History of the 1920s?* (Review Essay), 617-45; Knox, "Mussolini Unleashed, 1939-1941," 138; Lee and Petter, "The Colonial Office, War, and Development Policy," 119; Lewin, "The American Magic: Codes, Ciphers, and the Defeat of Japan," 211; Ose, "Entscheidung im Westen, 1944," 707; Prange, "At Dawn We Slept: The Untold Story of Pearl Harbor," 496; Terry, "Poland's Place in Europe: General Sikorski and the Origin of the Oder-Neisse Line, 1939-1943," 1297; Toland, "Infamy: Pearl Harbor and Its Aftermath," 496; Wistrich, "Who's Who in Nazi Germany," 704
 Wortman, Miles L., "Government and Society in Central America, 1680-1840," 780
 Wortmann, Michael, "Baldur von Schirach: Hitlers Jugendführer," 1288
 Woytak, Richard (R), 1297
 Wright, A. D., "The Counter-Reformation: Catholic Europe and the Non-Christian World," 698
 Wright, J. Leitch, Jr. (R), 478
 Wright, Marcia, and Margaret Jean Hay, editors, "African Women and the Law: Historical Perspectives" (E), 1123
 Wright, Thomas C. (R), 1351
 Wright, William E. (R), 134

- Wrigley, Chris, editor, "A History of British Industrial Relations, 1875–1914" (E), 515
- Wrigley, E. A., and R. S. Schofield, "The Population History of England, 1541–1871: A Reconstruction," 386
- "Writings on South Asian History and Society," edited by Guha (E), 1124
- Wyatt-Brown, Bertram (R), 1070
- Wyatt-Brown, Bertram, "Southern Honor: Ethics and Behavior in the Old South," 753
- Wylie, Raymond F., "The Emergence of Maoism: Mao Tse-tung, Ch'en Po-ta, and the Search for Chinese Theory, 1935–1945," 451
- Wylie, Turrell V. (R), 453, 1309
- Wynia, Gary W. (R), 513
- Wynjtes, Sherrin Marshall (R), 1278
- Yamashita, Tomoyuki, 1249
- "The Yamashita Precedent: War Crimes and Command Responsibility," by Lael, 1249
- Yaney, George (R), 1028
- Yang, Anand A. (R), 456
- Yang, Sung Chul, "Korea and Two Regimes: Kim Il Sung and Park Chung Hee," 160
- "Yankee Enterprise," edited by Mayr and Post, 468
- Yeager, Gertrude Matyoka, "Barros Arana's *Historia jeneral de Chile*: Politics, History, and National Identity," 229
- "The Years of Lyndon Johnson," vol. 1: "The Path to Power," by Caro, 1346
- Yerushalmi, Yosef Hayim, "Zakhor: Jewish History and Jewish Memory," 1239
- Young, Charles R. (R), 98
- Young, Crawford, "Ideology and Development in Africa," 445
- Young, David (R), 381
- Young, Ken, and Patricia L. Garside, "Metropolitan London: Politics and Urban Change, 1837–1981," 1267
- Young, Marilyn B., and William G. Rosenberg, "Transforming Russia and China: Revolutionary Struggle in the Twentieth Century," 79
- Young, Roger S., Jr., 953–54
- "Young Edward Gibbon," by Craddock, 110
- Young-Bruehl, Elisabeth, "Hannah Arendt: For Love of the World," 75
- YOUTH AND YOUTH MOVEMENTS: Eagan, "Class, Culture, and the Classroom," 207; Humphries, "Hooligans or Rebels?" 116; Wortmann, "Baldur von Schirach: Hitlers Jugendführer," 1288
- Yuzyk, Paul (R), 1110
- "Yves de Vallone," by O'Higgins, 699
- "Zabern 1913," by Schoenbaum, 131
- Zagoria, Donald S., editor, "Soviet Policy in East Asia" (E), 790
- Zagorin, Perez, "Rebels and Rulers, 1500–1600," vol. 1: "Society, States, and Early Modern Revolution: Agrarian and Urban Rebellions"; vol. 2: "Provincial Rebellion: Revolutionary Civil Wars, 1560–1660," 1241
- "Zakhor: Jewish History and Jewish Memory," by Yerushalmi, 1239
- "Zapadnaia Evropa v russkoi pis'mennosti, XV–XVI vekov," by Kazakova, 145
- "To zētēma ton trapezōn, 1871–1873," by Giōrgos, 431
- "Zhenshchiny—Revoliutsionery i uchenye," edited by Mints and Nenarokov, 721
- Zhongshu, Wang, "Han Civilization," 152
- Ziemke, Earl F. (R), 707
- Zimmermann, Harald, and Raymund Kottje, editors, "Hrabanus Maurus: Lehrer, Abt und Bischof," 969
- "Zionism in Poland," by Mendelsohn, 143
- "Zionism," by Vital, 80
- Zuckerman, Michael W., editor, "Friends and Neighbors: Group Life in America's First Plural Society," 1319
- Zunz, Olivier, "The Changing Face of Inequality: Urbanization, Industrial Development, and Immigrants in Detroit, 1880–1920," 1088
- "Zwischen Weltkrieg und Revolution," by Schek, 132

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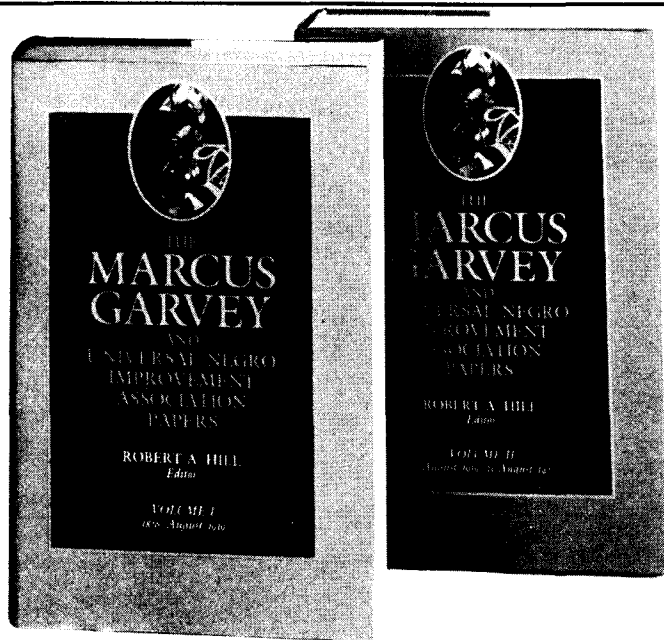
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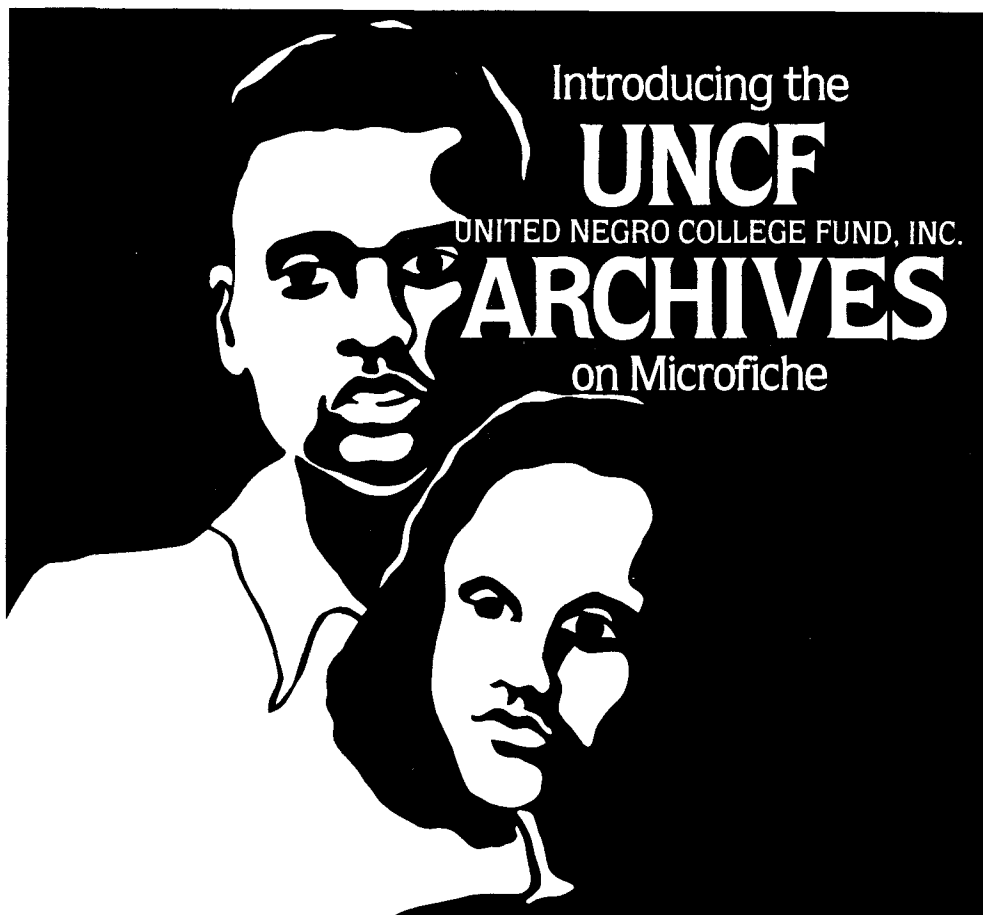
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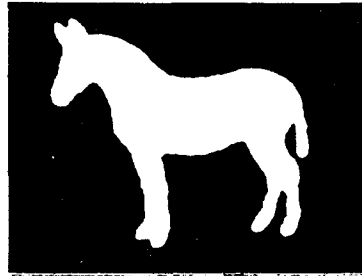
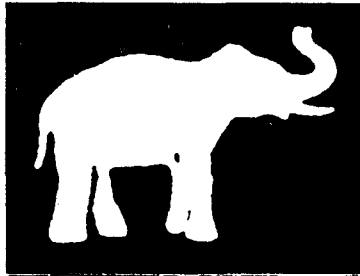
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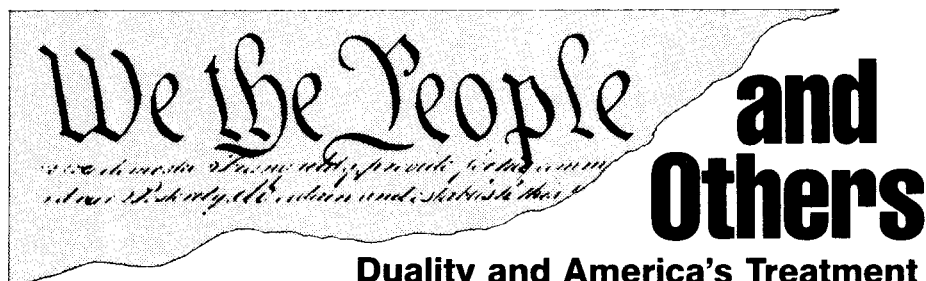


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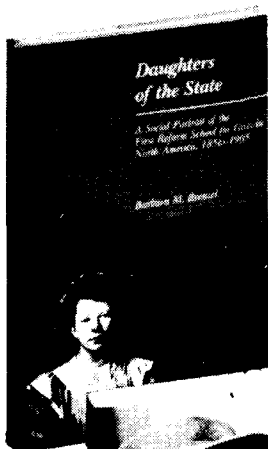
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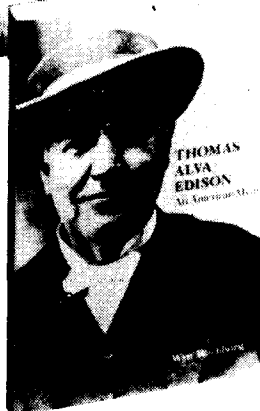
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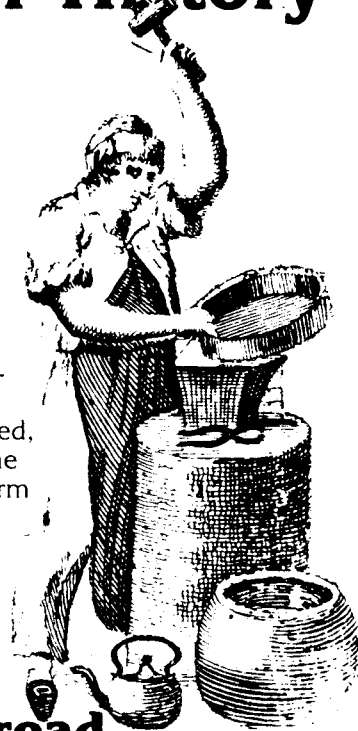
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